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Manuscripts and editorial correspondence relating to the regular quarterly issues of this REVIEW should be addressed to Bernard F. Haley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.

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## THE COST OF CAPITAL, CORPORATION FINANCE AND THE THEORY OF INVESTMENT

By FRANCO MODIGLIANI AND MERTON H. MILLER\*

What is the "cost of capital" to a firm in a world in which funds are used to acquire assets whose yields are uncertain; and in which capital can be obtained by many different media, ranging from pure debt instruments, representing money-fixed claims, to pure equity issues, giving holders only the right to a pro-rata share in the uncertain venture? This question has vexed at least three classes of economists: (1) the corporation finance specialist concerned with the techniques of financing firms so as to ensure their survival and growth; (2) the managerial economist concerned with capital budgeting; and (3) the economic theorist concerned with explaining investment behavior at both the micro and macro levels.<sup>1</sup>

In much of his formal analysis, the economic theorist at least has tended to side-step the essence of this cost-of-capital problem by proceeding as though physical assets—like bonds—could be regarded as yielding known, sure streams. Given this assumption, the theorist has concluded that the cost of capital to the owners of a firm is simply the rate of interest on bonds; and has derived the familiar proposition that the firm, acting rationally, will tend to push investment to the point

\* The authors are, respectively, professor and associate professor of economics in the Graduate School of Industrial Administration, Carnegie Institute of Technology. This article is a revised version of a paper delivered at the annual meeting of the Econometric Society, December 1956. The authors express thanks for the comments and suggestions made at that time by the discussants of the paper, Evsey Domar, Robert Eisner and John Lintner, and subsequently by James Duesenberry. They are also greatly indebted to many of their present and former colleagues and students at Carnegie Tech who served so often and with such remarkable patience as a critical forum for the ideas here presented.

<sup>1</sup> The literature bearing on the cost-of-capital problem is far too extensive for listing here. Numerous references to it will be found throughout the paper though we make no claim to completeness. One phase of the problem which we do not consider explicitly, but which has a considerable literature of its own is the relation between the cost of capital and public utility rates. For a recent summary of the "cost-of-capital theory" of rate regulation and a brief discussion of some of its implications, the reader may refer to H. M. Somers [20].

where the marginal yield on physical assets is equal to the market rate of interest.<sup>2</sup> This proposition can be shown to follow from either of two criteria of rational decision-making which are equivalent under certainty, namely (1) the maximization of profits and (2) the maximization of market value.

According to the first criterion, a physical asset is worth acquiring if it will increase the net profit of the owners of the firm. But net profit will increase only if the expected rate of return, or yield, of the asset exceeds the rate of interest. According to the second criterion, an asset is worth acquiring if it increases the value of the owners' equity, *i.e.*, if it adds more to the market value of the firm than the costs of acquisition. But what the asset adds is given by capitalizing the stream it generates at the market rate of interest, and this capitalized value will exceed its cost if and only if the yield of the asset exceeds the rate of interest. Note that, under either formulation, the cost of capital is equal to the rate of interest on bonds, regardless of whether the funds are acquired through debt instruments or through new issues of common stock. Indeed, in a world of sure returns, the distinction between debt and equity funds reduces largely to one of terminology.

It must be acknowledged that some attempt is usually made in this type of analysis to allow for the existence of uncertainty. This attempt typically takes the form of superimposing on the results of the certainty analysis the notion of a "risk discount" to be subtracted from the expected yield (or a "risk premium" to be added to the market rate of interest). Investment decisions are then supposed to be based on a comparison of this "risk adjusted" or "certainty equivalent" yield with the market rate of interest.<sup>3</sup> No satisfactory explanation has yet been provided, however, as to what determines the size of the risk discount and how it varies in response to changes in other variables.

Considered as a convenient approximation, the model of the firm constructed via this certainty—or certainty-equivalent—approach has admittedly been useful in dealing with some of the grosser aspects of the processes of capital accumulation and economic fluctuations. Such a model underlies, for example, the familiar Keynesian aggregate investment function in which aggregate investment is written as a function of the rate of interest—the same riskless rate of interest which appears later in the system in the liquidity-preference equation. Yet few would maintain that this approximation is adequate. At the macroeconomic level there are ample grounds for doubting that the rate of interest has

<sup>2</sup> Or, more accurately, to the marginal cost of borrowed funds since it is customary, at least in advanced analysis, to draw the supply curve of borrowed funds to the firm as a rising one. For an advanced treatment of the certainty case, see F. and V. Lutz [13].

<sup>3</sup> The classic examples of the certainty-equivalent approach are found in J. R. Hicks [8] and O. Lange [11].

as large and as direct an influence on the rate of investment as this analysis would lead us to believe. At the microeconomic level the certainty model has little descriptive value and provides no real guidance to the finance specialist or managerial economist whose main problems cannot be treated in a framework which deals so cavalierly with uncertainty and ignores all forms of financing other than debt issues.<sup>4</sup>

Only recently have economists begun to face up seriously to the problem of the cost of capital *cum* risk. In the process they have found their interests and endeavors merging with those of the finance specialist and the managerial economist who have lived with the problem longer and more intimately. In this joint search to establish the principles which govern rational investment and financial policy in a world of uncertainty two main lines of attack can be discerned. These lines represent, in effect, attempts to extrapolate to the world of uncertainty each of the two criteria—profit maximization and market value maximization—which were seen to have equivalent implications in the special case of certainty. With the recognition of uncertainty this equivalence vanishes. In fact, the profit maximization criterion is no longer even well defined. Under uncertainty there corresponds to each decision of the firm not a unique profit outcome, but a plurality of mutually exclusive outcomes which can at best be described by a subjective probability distribution. The profit outcome, in short, has become a random variable and as such its maximization no longer has an operational meaning. Nor can this difficulty generally be disposed of by using the mathematical expectation of profits as the variable to be maximized. For decisions which affect the expected value will also tend to affect the dispersion and other characteristics of the distribution of outcomes. In particular, the use of debt rather than equity funds to finance a given venture may well increase the expected return to the owners, but only at the cost of increased dispersion of the outcomes.

Under these conditions the profit outcomes of alternative investment and financing decisions can be compared and ranked only in terms of a *subjective* "utility function" of the owners which weighs the expected yield against other characteristics of the distribution. Accordingly, the extrapolation of the profit maximization criterion of the certainty model has tended to evolve into utility maximization, sometimes explicitly, more frequently in a qualitative and heuristic form.<sup>5</sup>

The utility approach undoubtedly represents an advance over the certainty or certainty-equivalent approach. It does at least permit us

<sup>4</sup> Those who have taken a "case-method" course in finance in recent years will recall in this connection the famous Liquigas case of Hunt and Williams, [9, pp. 193-96] a case which is often used to introduce the student to the cost-of-capital problem and to poke a bit of fun at the economist's certainty-model.

<sup>5</sup> For an attempt at a rigorous explicit development of this line of attack, see F. Modigliani and M. Zeman [14].

to explore (within limits) some of the implications of different financing arrangements, and it does give some meaning to the "cost" of different types of funds. However, because the cost of capital has become an essentially subjective concept, the utility approach has serious drawbacks for normative as well as analytical purposes. How, for example, is management to ascertain the risk preferences of its stockholders and to compromise among their tastes? And how can the economist build a meaningful investment function in the face of the fact that any given investment opportunity might or might not be worth exploiting depending on precisely who happen to be the owners of the firm at the moment?

Fortunately, these questions do not have to be answered; for the alternative approach, based on market value maximization, can provide the basis for an operational definition of the cost of capital and a workable theory of investment. Under this approach any investment project and its concomitant financing plan must pass only the following test: Will the project, as financed, raise the market value of the firm's shares? If so, it is worth undertaking; if not, its return is less than the marginal cost of capital to the firm. Note that such a test is entirely independent of the tastes of the current owners, since market prices will reflect not only their preferences but those of all potential owners as well. If any current stockholder disagrees with management and the market over the valuation of the project, he is free to sell out and reinvest elsewhere, but will still benefit from the capital appreciation resulting from management's decision.

The potential advantages of the market-value approach have long been appreciated; yet analytical results have been meager. What appears to be keeping this line of development from achieving its promise is largely the lack of an adequate theory of the effect of financial structure on market valuations, and of how these effects can be inferred from objective market data. It is with the development of such a theory and of its implications for the cost-of-capital problem that we shall be concerned in this paper.

Our procedure will be to develop in Section I the basic theory itself and to give some brief account of its empirical relevance. In Section II, we show how the theory can be used to answer the cost-of-capital question and how it permits us to develop a theory of investment of the firm under conditions of uncertainty. Throughout these sections the approach is essentially a partial-equilibrium one focusing on the firm and "industry." Accordingly, the "prices" of certain income streams will be treated as constant and given from outside the model, just as in the standard Marshallian analysis of the firm and industry the prices of all inputs and of all other products are taken as given. We have chosen to focus at this level rather than on the economy as a whole because it

is at the level of the firm and the industry that the interests of the various specialists concerned with the cost-of-capital problem come most closely together. Although the emphasis has thus been placed on partial-equilibrium analysis, the results obtained also provide the essential building blocks for a general equilibrium model which shows how those prices which are here taken as given, are themselves determined. For reasons of space, however, and because the material is of interest in its own right, the presentation of the general equilibrium model which rounds out the analysis must be deferred to a subsequent paper.

### I. *The Valuation of Securities, Leverage, and the Cost of Capital*

#### A. *The Capitalization Rate for Uncertain Streams*

As a starting point, consider an economy in which all physical assets are owned by corporations. For the moment, assume that these corporations can finance their assets by issuing common stock only; the introduction of bond issues, or their equivalent, as a source of corporate funds is postponed until the next part of this section.

The physical assets held by each firm will yield to the owners of the firm—its stockholders—a stream of “profits” over time; but the elements of this series need not be constant and in any event are uncertain. This stream of income, and hence the stream accruing to any share of common stock, will be regarded as extending indefinitely into the future. We assume, however, that the mean value of the stream over time, or average profit per unit of time, is finite and represents a random variable subject to a (subjective) probability distribution. We shall refer to the average value over time of the stream accruing to a given share as the return of that share; and to the mathematical expectation of this average as the expected return of the share.<sup>6</sup> Although individual investors may have different views as to the shape of the probability distri-

\* These propositions can be restated analytically as follows: The assets of the  $i$ th firm generate a stream:

$$X_i(1), X_i(2) \dots X_i(T)$$

whose elements are random variables subject to the joint probability distribution:

$$x_i[X_i(1), X_i(2) \dots X_i(T)].$$

The return to the  $i$ th firm is defined as:

$$X_i = \lim_{T \rightarrow \infty} \frac{1}{T} \sum_{t=1}^T X_i(t).$$

$X_i$  is itself a random variable with a probability distribution  $\phi_i(X_i)$  whose form is determined uniquely by  $x_i$ . The expected return  $\bar{X}_i$  is defined as  $\bar{X}_i = E(X_i) = \int X_i \phi_i(X_i) dX_i$ . If  $N_i$  is the number of shares outstanding, the return of the  $i$ th share is  $x_i = (1/N_i)X_i$  with probability distribution  $\phi_i(x_i)dx_i = \phi_i(N_i x_i)d(N_i x_i)$  and expected value  $\bar{x}_i = (1/N_i)\bar{X}_i$ .

bution of the return of any share, we shall assume for simplicity that they are at least in agreement as to the expected return.<sup>7</sup>

This way of characterizing uncertain streams merits brief comment. Notice first that the stream is a stream of profits, not dividends. As will become clear later, as long as management is presumed to be acting in the best interests of the stockholders, retained earnings can be regarded as equivalent to a fully subscribed, pre-emptive issue of common stock. Hence, for present purposes, the division of the stream between cash dividends and retained earnings in any period is a mere detail. Notice also that the uncertainty attaches to the mean value over time of the stream of profits and should not be confused with variability over time of the successive elements of the stream. That variability and uncertainty are two totally different concepts should be clear from the fact that the elements of a stream can be variable even though known with certainty. It can be shown, furthermore, that whether the elements of a stream are sure or uncertain, the effect of variability per se on the valuation of the stream is at best a second-order one which can safely be neglected for our purposes (and indeed most others too).<sup>8</sup>

The next assumption plays a strategic role in the rest of the analysis. We shall assume that firms can be divided into "equivalent return" classes such that the return on the shares issued by any firm in any given class is proportional to (and hence perfectly correlated with) the return on the shares issued by any other firm in the same class. This assumption implies that the various shares within the same class differ, at most, by a "scale factor." Accordingly, if we adjust for the difference in scale, by taking the *ratio* of the return to the expected return, the probability distribution of that ratio is identical for all shares in the class. It follows that all relevant properties of a share are uniquely characterized by specifying (1) the class to which it belongs and (2) its expected return.

The significance of this assumption is that it permits us to classify firms into groups within which the shares of different firms are "homogeneous," that is, perfect substitutes for one another. We have, thus, an analogue to the familiar concept of the industry in which it is the commodity produced by the firms that is taken as homogeneous. To complete this analogy with Marshallian price theory, we shall assume in the

<sup>7</sup> To deal adequately with refinements such as differences among investors in estimates of expected returns would require extensive discussion of the theory of portfolio selection. Brief references to these and related topics will be made in the succeeding article on the general equilibrium model.

<sup>8</sup> The reader may convince himself of this by asking how much he would be willing to rebate to his employer for the privilege of receiving his annual salary in equal monthly installments rather than in irregular amounts over the year. See also J. M. Keynes [10, esp. pp. 53-54].



analysis to follow that the shares concerned are traded in perfect markets under conditions of atomistic competition.<sup>9</sup>

From our definition of homogeneous classes of stock it follows that in equilibrium in a perfect capital market the price per dollar's worth of expected return must be the same for all shares of any given class. Or, equivalently, in any given class the price of every share must be proportional to its expected return. Let us denote this factor of proportionality for any class, say the  $k$ th class, by  $1/\rho_k$ . Then if  $p_j$  denotes the price and  $\bar{x}_j$  is the expected return per share of the  $j$ th firm in class  $k$ , we must have:

$$(1) \quad p_j = \frac{1}{\rho_k} \bar{x}_j;$$

or, equivalently,

$$(2) \quad \frac{\bar{x}_j}{p_j} = \rho_k \text{ a constant for all firms } j \text{ in class } k.$$

The constants  $\rho_k$  (one for each of the  $k$  classes) can be given several economic interpretations: (a) From (2) we see that each  $\rho_k$  is the expected rate of return of any share in class  $k$ . (b) From (1)  $1/\rho_k$  is the price which an investor has to pay for a dollar's worth of expected return in the class  $k$ . (c) Again from (1), by analogy with the terminology for perpetual bonds,  $\rho_k$  can be regarded as the market rate of capitalization for the expected value of the uncertain streams of the kind generated by the  $k$ th class of firms.<sup>10</sup>

### B. Debt Financing and Its Effects on Security Prices

Having developed an apparatus for dealing with uncertain streams we can now approach the heart of the cost-of-capital problem by dropping the assumption that firms cannot issue bonds. The introduction of debt-financing changes the market for shares in a very fundamental way. Because firms may have different proportions of debt in their capi-

<sup>9</sup> Just what our classes of stocks contain and how the different classes can be identified by outside observers are empirical questions to which we shall return later. For the present, it is sufficient to observe: (1) Our concept of a class, while not identical to that of the industry is at least closely related to it. Certainly the basic characteristics of the probability distributions of the returns on assets will depend to a significant extent on the product sold and the technology used. (2) What are the appropriate class boundaries will depend on the particular problem being studied. An economist concerned with general tendencies in the market, for example, might well be prepared to work with far wider classes than would be appropriate for an investor planning his portfolio, or a firm planning its financial strategy.

<sup>10</sup> We cannot, on the basis of the assumptions so far, make any statements about the relationship or spread between the various  $\rho$ 's or capitalization rates. Before we could do so we would have to make further specific assumptions about the way investors believe the probability distributions vary from class to class, as well as assumptions about investors' preferences as between the characteristics of different distributions.



tal structure, shares of different companies, even in the same class, can give rise to different probability distributions of returns. In the language of finance, the shares will be subject to different degrees of financial risk or "leverage" and hence they will no longer be perfect substitutes for one another.

To exhibit the mechanism determining the relative prices of shares under these conditions, we make the following two assumptions about the nature of bonds and the bond market, though they are actually stronger than is necessary and will be relaxed later: (1) All bonds (including any debts issued by households for the purpose of carrying shares) are assumed to yield a constant income per unit of time, and this income is regarded as certain by all traders regardless of the issuer. (2) Bonds, like stocks, are traded in a perfect market, where the term perfect is to be taken in its usual sense as implying that any two commodities which are perfect substitutes for each other must sell, in equilibrium, at the same price. It follows from assumption (1) that all bonds are in fact perfect substitutes up to a scale factor. It follows from assumption (2) that they must all sell at the same price per dollar's worth of return, or what amounts to the same thing must yield the same rate of return. This rate of return will be denoted by  $r$  and referred to as the rate of interest or, equivalently, as the capitalization rate for sure streams. We now can derive the following two basic propositions with respect to the valuation of securities in companies with different capital structures:

*Proposition I.* Consider any company  $j$  and let  $\bar{X}_j$  stand as before for the expected return on the assets owned by the company (that is, its expected profit before deduction of interest). Denote by  $D_j$  the market value of the debts of the company; by  $S_j$  the market value of its common shares; and by  $V_j \equiv S_j + D_j$  the market value of all its securities or, as we shall say, the market value of the firm. Then, our Proposition I asserts that we must have in equilibrium:

$$(3) \quad V_j \equiv (S_j + D_j) = \bar{X}_j / \rho_k, \text{ for any firm } j \text{ in class } k.$$

That is, the market value of any firm is independent of its capital structure and is given by capitalizing its expected return at the rate  $\rho_k$  appropriate to its class.

This proposition can be stated in an equivalent way in terms of the firm's "average cost of capital,"  $\bar{X}_j / V_j$ , which is the ratio of its expected return to the market value of all its securities. Our proposition then is:

$$(4) \quad \frac{\bar{X}_j}{(S_j + D_j)} \equiv \frac{\bar{X}_j}{V_j} = \rho_k, \text{ for any firm } j, \text{ in class } k.$$

That is, the average cost of capital to any firm is completely independent of

its capital structure and is equal to the capitalization rate of a pure equity stream of its class.

To establish Proposition I we will show that as long as the relations (3) or (4) do not hold between any pair of firms in a class, arbitrage will take place and restore the stated equalities. We use the term arbitrage advisedly. For if Proposition I did not hold, an investor could buy and sell stocks and bonds in such a way as to exchange one income stream for another stream, identical in all relevant respects but selling at a lower price. The exchange would therefore be advantageous to the investor quite independently of his attitudes toward risk.<sup>11</sup> As investors exploit these arbitrage opportunities, the value of the overpriced shares will fall and that of the underpriced shares will rise, thereby tending to eliminate the discrepancy between the market values of the firms.

By way of proof, consider two firms in the same class and assume for simplicity only, that the expected return,  $\bar{X}$ , is the same for both firms. Let company 1 be financed entirely with common stock while company 2 has some debt in its capital structure. Suppose first the value of the levered firm,  $V_2$ , to be larger than that of the unlevered one,  $V_1$ . Consider an investor holding  $s_2$  dollars' worth of the shares of company 2, representing a fraction  $\alpha$  of the total outstanding stock,  $S_2$ . The return from this portfolio, denoted by  $Y_2$ , will be a fraction  $\alpha$  of the income available for the stockholders of company 2, which is equal to the total return  $X_2$  less the interest charge,  $rD_2$ . Since under our assumption of homogeneity, the anticipated total return of company 2,  $X_2$ , is, under all circumstances, the same as the anticipated total return to company 1,  $X_1$ , we can hereafter replace  $X_2$  and  $X_1$  by a common symbol  $X$ . Hence, the return from the initial portfolio can be written as:

$$(5) \quad Y_2 = \alpha(X - rD_2).$$

Now suppose the investor sold his  $\alpha S_2$  worth of company 2 shares and acquired instead an amount  $s_1 = \alpha(S_2 + D_2)$  of the shares of company 1. He could do so by utilizing the amount  $\alpha S_2$  realized from the sale of his initial holding and borrowing an additional amount  $\alpha D_2$  on his own credit, pledging his new holdings in company 1 as a collateral. He would thus secure for himself a fraction  $s_1/S_1 = \alpha(S_2 + D_2)/S_1$  of the shares and earnings of company 1. Making proper allowance for the interest payments on his personal debt  $\alpha D_2$ , the return from the new portfolio,  $Y_1$ , is given by:

<sup>11</sup> In the language of the theory of choice, the exchanges are movements from inefficient points in the interior to efficient points on the boundary of the investor's opportunity set; and not movements between efficient points along the boundary. Hence for this part of the analysis nothing is involved in the way of specific assumptions about investor attitudes or behavior other than that investors behave consistently and prefer more income to less income, *ceteris paribus*.

$$(6) \quad Y_1 = \frac{\alpha(S_2 + D_2)}{S_1} X - r\alpha D_2 = \alpha \frac{V_2}{V_1} X - r\alpha D_2.$$

Comparing (5) with (6) we see that as long as  $V_2 > V_1$  we must have  $Y_1 > Y_2$ , so that it pays owners of company 2's shares to sell their holdings, thereby depressing  $S_2$  and hence  $V_2$ ; and to acquire shares of company 1, thereby raising  $S_1$  and thus  $V_1$ . We conclude therefore that levered companies cannot command a premium over unlevered companies because investors have the opportunity of putting the equivalent leverage into their portfolio directly by borrowing on personal account.

Consider now the other possibility, namely that the market value of the levered company  $V_2$  is less than  $V_1$ . Suppose an investor holds initially an amount  $s_1$  of shares of company 1, representing a fraction  $\alpha$  of the total outstanding stock,  $S_1$ . His return from this holding is:

$$Y_1 = \frac{s_1}{S_1} X = \alpha X.$$

Suppose he were to exchange this initial holding for another portfolio, also worth  $s_1$ , but consisting of  $s_2$  dollars of stock of company 2 and of  $d$  dollars of bonds, where  $s_2$  and  $d$  are given by:

$$(7) \quad s_2 = \frac{S_2}{V_2} s_1, \quad d = \frac{D_2}{V_2} s_1.$$

In other words the new portfolio is to consist of stock of company 2 and of bonds in the proportions  $S_2/V_2$  and  $D_2/V_2$ , respectively. The return from the stock in the new portfolio will be a fraction  $s_2/S_2$  of the total return to stockholders of company 2, which is  $(X - rD_2)$ , and the return from the bonds will be  $rd$ . Making use of (7), the total return from the portfolio,  $Y_2$ , can be expressed as follows:

$$Y_2 = \frac{s_2}{S_2} (X - rD_2) + rd = \frac{s_1}{V_2} (X - rD_2) + r \frac{D_2}{V_2} s_1 = \frac{s_1}{V_2} X = \alpha \frac{S_1}{V_2} X$$

(since  $s_1 = \alpha S_1$ ). Comparing  $Y_2$  with  $Y_1$  we see that, if  $V_2 < S_1 \equiv V_1$ , then  $Y_2$  will exceed  $Y_1$ . Hence it pays the holders of company 1's shares to sell these holdings and replace them with a mixed portfolio containing an appropriate fraction of the shares of company 2.

The acquisition of a mixed portfolio of stock of a levered company  $j$  and of bonds in the proportion  $S_j/V_j$  and  $D_j/V_j$ , respectively, may be regarded as an operation which "undoes" the leverage, giving access to an appropriate fraction of the unlevered return  $X_j$ . It is this possibility of undoing leverage which prevents the value of levered firms from being consistently less than those of unlevered firms, or more generally prevents the average cost of capital  $\bar{X}_j/V_j$  from being systematically higher for levered than for nonlevered companies in the same class.

Since we have already shown that arbitrage will also prevent  $V_2$  from being larger than  $V_1$ , we can conclude that in equilibrium we must have  $V_2 = V_1$ , as stated in Proposition I.

*Proposition II.* From Proposition I we can derive the following proposition concerning the rate of return on common stock in companies whose capital structure includes some debt: the expected rate of return or yield,  $i$ , on the stock of any company  $j$  belonging to the  $k$ th class is a linear function of leverage as follows:

$$(8) \quad i_j = \rho_k + (\rho_k - r) D_j / S_j.$$

That is, the expected yield of a share of stock is equal to the appropriate capitalization rate  $\rho_k$  for a pure equity stream in the class, plus a premium related to financial risk equal to the debt-to-equity ratio times the spread between  $\rho_k$  and  $r$ . Or equivalently, the market price of any share of stock is given by capitalizing its expected return at the continuously variable rate  $i_j$  of (8).<sup>12</sup>

A number of writers have stated close equivalents of our Proposition I although by appealing to intuition rather than by attempting a proof and only to insist immediately that the results were not applicable to the actual capital markets.<sup>13</sup> Proposition II, however, so far as we have been able to discover is new.<sup>14</sup> To establish it we first note that, by definition, the expected rate of return,  $i$ , is given by:

$$(9) \quad i_j \equiv \frac{\bar{X}_j - r D_j}{S_j}.$$

From Proposition I, equation (3), we know that:

$$\bar{X}_j = \rho_k (S_j + D_j).$$

Substituting in (9) and simplifying, we obtain equation (8).

<sup>12</sup> To illustrate, suppose  $\bar{X} = 1000$ ,  $D = 4000$ ,  $r = 5$  per cent and  $\rho_k = 10$  per cent. These values imply that  $V = 10,000$  and  $S = 6000$  by virtue of Proposition I. The expected yield or rate of return per share is then:

$$i = \frac{1000 - 200}{6000} = .1 + (.1 - .05) \frac{4000}{6000} = 13\frac{1}{3} \text{ per cent.}$$

<sup>13</sup> See, for example, J. B. Williams [21, esp. pp. 72-73]; David Durand [3]; and W. A. Morton [15]. None of these writers describe in any detail the mechanism which is supposed to keep the average cost of capital constant under changes in capital structure. They seem, however, to be visualizing the equilibrating mechanism in terms of switches by investors between stocks and bonds as the yields of each get out of line with their "riskiness." This is an argument quite different from the pure arbitrage mechanism underlying our proof, and the difference is crucial. Regarding Proposition I as resting on investors' attitudes toward risk leads inevitably to a misunderstanding of many factors influencing relative yields such as, for example, limitations on the portfolio composition of financial institutions. See below, esp. Section I.D.

<sup>14</sup> Morton does make reference to a linear yield function but only "... for the sake of simplicity and because the particular function used makes no essential difference in my conclusions" [15, p. 443, note 2].

### C. Some Qualifications and Extensions of the Basic Propositions

The methods and results developed so far can be extended in a number of useful directions, of which we shall consider here only three: (1) allowing for a corporate profits tax under which interest payments are deductible; (2) recognizing the existence of a multiplicity of bonds and interest rates; and (3) acknowledging the presence of market imperfections which might interfere with the process of arbitrage. The first two will be examined briefly in this section with some further attention given to the tax problem in Section II. Market imperfections will be discussed in Part D of this section in the course of a comparison of our results with those of received doctrines in the field of finance.

*Effects of the Present Method of Taxing Corporations.* The deduction of interest in computing taxable corporate profits will prevent the arbitrage process from making the value of all firms in a given class proportional to the expected returns generated by their physical assets. Instead, it can be shown (by the same type of proof used for the original version of Proposition I) that the market values of firms in each class must be proportional in equilibrium to their expected return net of taxes (that is, to the sum of the interest paid and expected net stockholder income). This means we must replace each  $\bar{X}_j$  in the original versions of Propositions I and II with a new variable  $\bar{X}_j^*$  representing the total income net of taxes generated by the firm:

$$(10) \quad \bar{X}_j^* \equiv (\bar{X}_j - rD_j)(1 - \tau) + rD_j \equiv \bar{\pi}_j^* + rD_j,$$

where  $\bar{\pi}_j^*$  represents the expected net income accruing to the common stockholders and  $\tau$  stands for the average rate of corporate income tax.<sup>15</sup>

After making these substitutions, the propositions, when adjusted for taxes, continue to have the same form as their originals. That is, Proposition I becomes:

$$(11) \quad \frac{\bar{X}_j^*}{V_j} = \rho_k^*, \text{ for any firm in class } k,$$

and Proposition II becomes

$$(12) \quad i_j \equiv \frac{\bar{\pi}_j^*}{S_j} = \rho_j^* + (\rho_k^* - r)D_j/S_j$$

where  $\rho_k^*$  is the capitalization rate for income net of taxes in class  $k$ .

Although the form of the propositions is unaffected, certain interpretations must be changed. In particular, the after-tax capitalization rate

<sup>15</sup> For simplicity, we shall ignore throughout the tiny element of progression in our present corporate tax and treat  $\tau$  as a constant independent of  $(X_j - rD_j)$ .

$\rho_k^*$  can no longer be identified with the "average cost of capital" which is  $\rho_k = \bar{X}_j/V_j$ . The difference between  $\rho_k^*$  and the "true" average cost of capital, as we shall see, is a matter of some relevance in connection with investment planning within the firm (Section II). For the description of market behavior, however, which is our immediate concern here, the distinction is not essential. To simplify presentation, therefore, and to preserve continuity with the terminology in the standard literature we shall continue in this section to refer to  $\rho_k^*$  as the average cost of capital, though strictly speaking this identification is correct only in the absence of taxes.

*Effects of a Plurality of Bonds and Interest Rates.* In existing capital markets we find not one, but a whole family of interest rates varying with maturity, with the technical provisions of the loan and, what is most relevant for present purposes, with the financial condition of the borrower.<sup>16</sup> Economic theory and market experience both suggest that the yields demanded by lenders tend to increase with the debt-equity ratio of the borrowing firm (or individual). If so, and if we can assume as a first approximation that this yield curve,  $r = r(D/S)$ , whatever its precise form, is the same for all borrowers, then we can readily extend our propositions to the case of a rising supply curve for borrowed funds.<sup>17</sup>

Proposition I is actually unaffected in form and interpretation by the fact that the rate of interest may rise with leverage; while the average cost of *borrowed* funds will tend to increase as debt rises, the average cost of funds from *all* sources will still be independent of leverage (apart from the tax effect). This conclusion follows directly from the ability of those who engage in arbitrage to undo the leverage in any financial structure by acquiring an appropriately mixed portfolio of bonds and stocks. Because of this ability, the ratio of earnings (*before* interest charges) to market value—i.e., the average cost of capital from all

<sup>16</sup> We shall not consider here the extension of the analysis to encompass the time structure of interest rates. Although some of the problems posed by the time structure can be handled within our comparative statics framework, an adequate discussion would require a separate paper.

<sup>17</sup> We can also develop a theory of bond valuation along lines essentially parallel to those followed for the case of shares. We conjecture that the curve of bond yields as a function of leverage will turn out to be a nonlinear one in contrast to the linear function of leverage developed for common shares. However, we would also expect that the rate of increase in the yield on new issues would not be substantial in practice. This relatively slow rise would reflect the fact that interest rate increases by themselves can never be completely satisfactory to creditors as compensation for their increased risk. Such increases may simply serve to raise  $r$  so high relative to  $\rho$  that they become self-defeating by giving rise to a situation in which even normal fluctuations in earnings may force the company into bankruptcy. The difficulty of borrowing more, therefore, tends to show up in the usual case not so much in higher rates as in the form of increasingly stringent restrictions imposed on the company's management and finances by the creditors; and ultimately in a complete inability to obtain new borrowed funds, at least from the institutional investors who normally set the standards in the market for bonds.



sources—must be the same for all firms in a given class.<sup>18</sup> In other words, the increased cost of borrowed funds as leverage increases will tend to be offset by a corresponding reduction in the yield of common stock. This seemingly paradoxical result will be examined more closely below in connection with Proposition II.

A significant modification of Proposition I would be required only if the yield curve  $r=r(D/S)$  were different for different borrowers, as might happen if creditors had marked preferences for the securities of a particular class of debtors. If, for example, corporations as a class were able to borrow at lower rates than individuals having equivalent personal leverage, then the average cost of capital to corporations might fall slightly, as leverage increased over some range, in reflection of this differential. In evaluating this possibility, however, remember that the relevant interest rate for our arbitrage operators is the rate on brokers' loans and, historically, that rate has not been noticeably higher than representative corporate rates.<sup>19</sup> The operations of holding companies and investment trusts which can borrow on terms comparable to operating companies represent still another force which could be expected to wipe out any marked or prolonged advantages from holding levered stocks.<sup>20</sup>

Although Proposition I remains unaffected as long as the yield curve is the same for all borrowers, the relation between common stock yields and leverage will no longer be the strictly linear one given by the original Proposition II. If  $r$  increases with leverage, the yield  $i$  will still tend to

<sup>18</sup> One normally minor qualification might be noted. Once we relax the assumption that all bonds have certain yields, our arbitrage operator faces the danger of something comparable to "gambler's ruin." That is, there is always the possibility that an otherwise sound concern—one whose long-run expected income is greater than its interest liability—might be forced into liquidation as a result of a run of temporary losses. Since reorganization generally involves costs, and because the operation of the firm may be hampered during the period of reorganization with lasting unfavorable effects on earnings prospects, we might perhaps expect heavily levered companies to sell at a slight discount relative to less heavily indebted companies of the same class.

<sup>19</sup> Under normal conditions, moreover, a substantial part of the arbitrage process could be expected to take the form, not of having the arbitrage operators go into debt on personal account to put the required leverage into their portfolios, but simply of having them reduce the amount of corporate bonds they already hold when they acquire underpriced unlevered stock. Margin requirements are also somewhat less of an obstacle to maintaining any desired degree of leverage in a portfolio than might be thought at first glance. Leverage could be largely restored in the face of higher margin requirements by switching to stocks having more leverage at the corporate level.

<sup>20</sup> An extreme form of inequality between borrowing and lending rates occurs, of course, in the case of preferred stocks, which can not be directly issued by individuals on personal account. Here again, however, we would expect that the operations of investment corporations plus the ability of arbitrage operators to sell off their holdings of preferred stocks would act to prevent the emergence of any substantial premiums (for this reason) on capital structures containing preferred stocks. Nor are preferred stocks so far removed from bonds as to make it impossible for arbitrage operators to approximate closely the risk and leverage of a corporate preferred stock by incurring a somewhat smaller debt on personal account.



rise as  $D/S$  increases, but at a decreasing rather than a constant rate. Beyond some high level of leverage, depending on the exact form of the interest function, the yield may even start to fall.<sup>21</sup> The relation between  $i$  and  $D/S$  could conceivably take the form indicated by the curve  $MD$

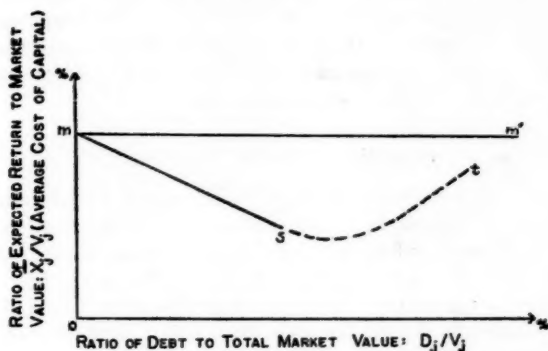


FIGURE 1

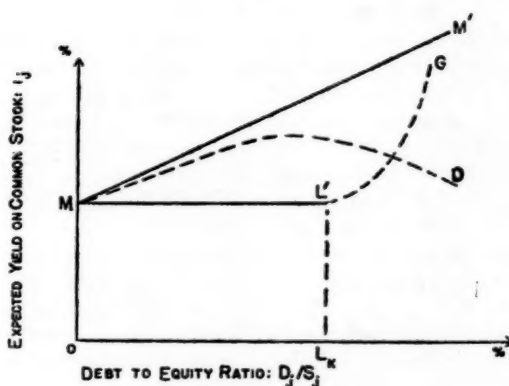


FIGURE 2

in Figure 2, although in practice the curvature would be much less pronounced. By contrast, with a constant rate of interest, the relation would be linear throughout as shown by line  $MM'$ , Figure 2.

The downward sloping part of the curve  $MD$  perhaps requires some

<sup>21</sup> Since new lenders are unlikely to permit this much leverage (*cf.* note 17), this range of the curve is likely to be occupied by companies whose earnings prospects have fallen substantially since the time when their debts were issued.

comment since it may be hard to imagine why investors, other than those who like lotteries, would purchase stocks in this range. Remember, however, that the yield curve of Proposition II is a consequence of the more fundamental Proposition I. Should the demand by the risk-lovers prove insufficient to keep the market to the peculiar yield-curve  $MD$ , this demand would be reinforced by the action of arbitrage operators. The latter would find it profitable to own a pro-rata share of the firm as a whole by holding its stock *and* bonds, the lower yield of the shares being thus offset by the higher return on bonds.

### D. *The Relation of Propositions I and II to Current Doctrines*

The propositions we have developed with respect to the valuation of firms and shares appear to be substantially at variance with current doctrines in the field of finance. The main differences between our view and the current view are summarized graphically in Figures 1 and 2. Our Proposition I [equation (4)] asserts that the average cost of capital,  $\bar{X}_j/V_j$ , is a constant for all firms  $j$  in class  $k$ , independently of their financial structure. This implies that, if we were to take a sample of firms in a given class, and if for each firm we were to plot the ratio of expected return to market value against some measure of leverage or financial structure, the points would tend to fall on a horizontal straight line with intercept  $\rho_k^*$ , like the solid line  $mm'$  in Figure 1.<sup>22</sup> From Proposition I we derived Proposition II [equation (8)] which, taking the simplest version with  $r$  constant, asserts that, for all firms in a class, the relation between the yield on common stock and financial structure, measured by  $D_j/S_j$ , will approximate a straight line with slope  $(\rho_k^* - r)$  and intercept  $\rho_k^*$ . This relationship is shown as the solid line  $MM'$  in Figure 2, to which reference has been made earlier.<sup>23</sup>

By contrast, the conventional view among finance specialists appears to start from the proposition that, other things equal, the earnings-price ratio (or its reciprocal, the times-earnings multiplier) of a firm's common stock will normally be only slightly affected by "moderate" amounts of debt in the firm's capital structure.<sup>24</sup> Translated into our no-

<sup>22</sup> In Figure 1 the measure of leverage used is  $D_j/V_j$  (the ratio of debt to market value) rather than  $D_j/S_j$  (the ratio of debt to equity), the concept used in the analytical development. The  $D_j/V_j$  measure is introduced at this point because it simplifies comparison and contrast of our view with the traditional position.

<sup>23</sup> The line  $MM'$  in Figure 2 has been drawn with a positive slope on the assumption that  $\rho_k^* > r$ , a condition which will normally obtain. Our Proposition II as given in equation (8) would continue to be valid, of course, even in the unlikely event that  $\rho_k^* < r$ , but the slope of  $MM'$  would be negative.

<sup>24</sup> See, e.g., Graham and Dodd [6, pp. 464-66]. Without doing violence to this position, we can bring out its implications more sharply by ignoring the qualification and treating the yield as a virtual constant over the relevant range. See in this connection the discussion in Durand [3, esp. pp. 225-37] of what he calls the "net income method" of valuation.

tation, it asserts that for any firm  $j$  in the class  $k$ ,

$$(13) \quad \frac{\bar{X}_j^r - rD_j}{S_j} = \frac{\pi_j^r}{S_j} = i_k^*, \text{ a constant for } \frac{D_j}{S_j} \leq L_k$$

or, equivalently,

$$(14) \quad S_j = \pi_j^r / i_k^*.$$

Here  $i_k^*$  represents the capitalization rate or earnings-price ratio on the common stock and  $L_k$  denotes some amount of leverage regarded as the maximum "reasonable" amount for firms of the class  $k$ . This assumed relationship between yield and leverage is the horizontal solid line  $ML'$  of Figure 2. Beyond  $L'$ , the yield will presumably rise sharply as the market discounts "excessive" trading on the equity. This possibility of a rising range for high leverages is indicated by the broken-line segment  $L'G$  in the figure.<sup>25</sup>

If the value of shares were really given by (14) then the over-all market value of the firm must be:

$$(16) \quad V_j = S_j + D_j = \frac{\bar{X}_j^r - rD_j}{i_k^*} + D_j = \frac{\bar{X}_j^r}{i_k^*} + \frac{(i_k^* - r)D_j}{i_k^*}.$$

That is, for any given level of expected total returns after taxes ( $\bar{X}_j^r$ ) and assuming, as seems natural, that  $i_k^* > r$ , the value of the firm must tend to *rise* with debt,<sup>26</sup> whereas our Proposition I asserts that the value of the firm is completely independent of the capital structure. Another way of contrasting our position with the traditional one is in terms of the cost of capital. Solving (16) for  $\bar{X}_j^r/V_j$  yields:

$$(17) \quad \bar{X}_j^r/V_j = i_k^* - (i_k^* - r)D_j/V_j.$$

According to this equation, the average cost of capital is not independent of capital structure as we have argued, but should tend to *fall* with increasing leverage, at least within the relevant range of moderate debt ratios, as shown by the line  $ms$  in Figure 1. Or to put it in more familiar terms, debt-financing should be "cheaper" than equity-financing if not carried too far.

When we also allow for the possibility of a rising range of stock yields for large values of leverage, we obtain a U-shaped curve like  $mst$  in

<sup>25</sup> To make it easier to see some of the implications of this hypothesis as well as to prepare the ground for later statistical testing, it will be helpful to assume that the notion of a critical limit on leverage beyond which yields rise rapidly, can be epitomized by a quadratic relation of the form:

$$(15) \quad \pi_j^r/S_j = i_k^* + \beta(D_j/S_j) + \alpha(D_j/S_j)^2, \quad \alpha > 0.$$

<sup>26</sup> For a typical discussion of how a promoter can, supposedly, increase the market value of a firm by recourse to debt issues, see W. J. Eiteman [4, esp. pp. 11-13].

Figure 1.<sup>27</sup> That a yield-curve for stocks of the form  $ML'G$  in Figure 2 implies a U-shaped cost-of-capital curve has, of course, been recognized by many writers. A natural further step has been to suggest that the capital structure corresponding to the trough of the U is an "optimal capital structure" towards which management ought to strive in the best interests of the stockholders.<sup>28</sup> According to our model, by contrast, no such optimal structure exists—all structures being equivalent from the point of view of the cost of capital.

Although the falling, or at least U-shaped, cost-of-capital function is in one form or another the dominant view in the literature, the ultimate rationale of that view is by no means clear. The crucial element in the position—that the expected earnings-price ratio of the stock is largely unaffected by leverage up to some conventional limit—is rarely even regarded as something which requires explanation. It is usually simply taken for granted or it is merely asserted that this is the way the market behaves.<sup>29</sup> To the extent that the constant earnings-price ratio has a rationale at all we suspect that it reflects in most cases the feeling that moderate amounts of debt in "sound" corporations do not really add very much to the "riskiness" of the stock. Since the extra risk is slight, it seems natural to suppose that firms will not have to pay noticeably higher yields in order to induce investors to hold the stock.<sup>30</sup>

A more sophisticated line of argument has been advanced by David Durand [3, pp. 231-33]. He suggests that because insurance companies and certain other important institutional investors are restricted to debt securities, nonfinancial corporations are able to borrow from them at interest rates which are lower than would be required to compensate

<sup>27</sup> The U-shaped nature of the cost-of-capital curve can be exhibited explicitly if the yield curve for shares as a function of leverage can be approximated by equation (15) of footnote 25. From that equation, multiplying both sides by  $S_j$  we obtain:  $\bar{\pi}_j r = \bar{X}_j r - r D_j = i_k^* S_j + \beta D_j + \alpha D_j^2 / S_j$  or, adding and subtracting  $i_k^* D_j$  from the right-hand side and collecting terms,

$$(18) \quad \bar{X}_j r = i_k^* (S_j + D_j) + (\beta + r - i_k^*) D_j + \alpha D_j^2 / S_j.$$

Dividing (18) by  $V_j$  gives an expression for the cost of capital:

$$(19) \quad \bar{X}_j r / V_j = i_k^* - (i_k^* - r - \beta) D_j / V_j + \alpha D_j^2 / S_j V_j = i_k^* - (i_k^* - r - \beta) D_j / V_j + \alpha (D_j / V_j)^2 / (1 - D_j / V_j)$$

which is clearly U-shaped since  $\alpha$  is supposed to be positive.

<sup>28</sup> For a typical statement see S. M. Robbins [16, p. 307]. See also Graham and Dodd [6, pp. 468-74].

<sup>29</sup> See e.g., Graham and Dodd [6, p. 466].

<sup>30</sup> A typical statement is the following by Guthmann and Dougall [7, p. 245]: "Theoretically it might be argued that the increased hazard from using bonds and preferred stocks would counterbalance this additional income and so prevent the common stock from being more attractive than when it had a lower return but fewer prior obligations. In practice, the extra earnings from 'trading on the equity' are often regarded by investors as more than sufficient to serve as a 'premium for risk' when the proportions of the several securities are judiciously mixed."

creditors in a free market. Thus, while he would presumably agree with our conclusions that stockholders could not gain from leverage in an unconstrained market, he concludes that they can gain under present institutional arrangements. This gain would arise by virtue of the "safety superpremium" which lenders are willing to pay corporations for the privilege of lending.<sup>31</sup>

The defective link in both the traditional and the Durand version of the argument lies in the confusion between investors' subjective risk preferences and their objective market opportunities. Our Propositions I and II, as noted earlier, do not depend for their validity on any assumption about individual risk preferences. Nor do they involve any assertion as to what is an adequate compensation to investors for assuming a given degree of risk. They rely merely on the fact that a given commodity cannot consistently sell at more than one price in the market; or more precisely that the price of a commodity representing a "bundle" of two other commodities cannot be consistently different from the weighted average of the prices of the two components (the weights being equal to the proportion of the two commodities in the bundle).

An analogy may be helpful at this point. The relations between  $1/\rho_k$ , the price per dollar of an unlevered stream in class  $k$ ;  $1/r$ , the price per dollar of a sure stream, and  $1/i_j$ , the price per dollar of a levered stream  $j$ , in the  $k$ th class, are essentially the same as those between, respectively, the price of whole milk, the price of butter fat, and the price of milk which has been thinned out by skimming off some of the butter fat. Our Proposition I states that a firm cannot reduce the cost of capital—i.e., increase the market value of the stream it generates—by securing part of its capital through the sale of bonds, even though debt money appears to be cheaper. This assertion is equivalent to the proposition that, under perfect markets, a dairy farmer cannot in general earn more for the milk he produces by skimming some of the butter fat and selling it separately, even though butter fat per unit weight, sells for more than whole milk. The advantage from skimming the milk rather than selling whole milk would be purely illusory; for what would be gained from selling the high-priced butter fat would be lost in selling the low-priced residue of thinned milk. Similarly our Proposition II—that the price per dollar of a levered stream falls as leverage increases—is an ex-

<sup>31</sup> Like Durand, Morton [15] contends "that the actual market deviates from [Proposition I] by giving a changing over-all cost of money at different points of the [leverage] scale" (p. 443, note 2, inserts ours), but the basis for this contention is nowhere clearly stated. Judging by the great emphasis given to the lack of mobility of investment funds between stocks and bonds and to the psychological and institutional pressures toward debt portfolios (see pp. 444-51 and especially his discussion of the optimal capital structure on p. 453) he would seem to be taking a position very similar to that of Durand above.

act analogue of the statement that the price per gallon of thinned milk falls continuously as more butter fat is skimmed off.<sup>32</sup>

It is clear that this last assertion is true as long as butter fat is worth more per unit weight than whole milk, and it holds even if, for many consumers, taking a little cream out of the milk (adding a little leverage to the stock) does not detract noticeably from the taste (does not add noticeably to the risk). Furthermore the argument remains valid even in the face of institutional limitations of the type envisaged by Durand. For suppose that a large fraction of the population habitually dines in restaurants which are required by law to serve only cream in lieu of milk (entrust their savings to institutional investors who can only buy bonds). To be sure the price of butter fat will then tend to be higher in relation to that of skimmed milk than in the absence such restrictions (the rate of interest will tend to be lower), and this will benefit people who eat at home and who like skim milk (who manage their own portfolio and are able and willing to take risk). But it will still be the case that a farmer cannot gain by skimming some of the butter fat and selling it separately (firm cannot reduce the cost of capital by recourse to borrowed funds).<sup>33</sup>

Our propositions can be regarded as the extension of the classical theory of markets to the particular case of the capital markets. Those who hold the current view—whether they realize it or not—must as-

<sup>32</sup> Let  $M$  denote the quantity of whole milk,  $B/M$  the proportion of butter fat in the whole milk, and let  $p_M$ ,  $p_B$  and  $p_a$  denote, respectively, the price per unit weight of whole milk, butter fat and thinned milk from which a fraction  $\alpha$  of the butter fat has been skimmed off. We then have the fundamental perfect market relation:

$$(a) \quad p_a(M - \alpha B) + p_B \alpha B = p_M M, \quad 0 \leq \alpha \leq 1,$$

stating that total receipts will be the same amount  $p_M M$ , independently of the amount  $\alpha B$  of butter fat that may have been sold separately. Since  $p_M$  corresponds to  $1/\rho$ ,  $p_B$  to  $1/r$ ,  $p_a$  to  $1/i$ ,  $M$  to  $\bar{X}$  and  $\alpha B$  to  $rD$ , (a) is equivalent to Proposition I,  $S + D = \bar{X}/\rho$ . From (a) we derive:

$$(b) \quad p_a = p_M \frac{M}{M - \alpha B} - p_B \frac{\alpha B}{M - \alpha B}$$

which gives the price of thinned milk as an explicit function of the proportion of butter fat skimmed off; the function decreasing as long as  $p_B > p_M$ . From (a) also follows:

$$(c) \quad 1/p_a = 1/p_M + (1/p_M - 1/p_B) \frac{p_B \alpha B}{p_a(M - \alpha B)}$$

which is the exact analogue of Proposition II, as given by (8).

<sup>33</sup> The reader who likes parables will find that the analogy with interrelated commodity markets can be pushed a good deal farther than we have done in the text. For instance, the effect of changes in the market rate of interest on the over-all cost of capital is the same as the effect of a change in the price of butter on the price of whole milk. Similarly, just as the relation between the prices of skim milk and butter fat influences the kind of cows that will be reared, so the relation between  $i$  and  $r$  influences the kind of ventures that will be undertaken. If people like butter we shall have Guernseys; if they are willing to pay a high price for safety, this will encourage ventures which promise smaller but less uncertain streams per dollar of physical assets.

sume not merely that there are lags and frictions in the equilibrating process—a feeling we certainly share,<sup>34</sup> claiming for our propositions only that they describe the central tendency around which observations will scatter—but also that there are large and *systematic* imperfections in the market which permanently bias the outcome. This is an assumption that economists, at any rate, will instinctively eye with some skepticism.

In any event, whether such prolonged, systematic departures from equilibrium really exist or whether our propositions are better descriptions of long-run market behavior can be settled only by empirical research. Before going on to the theory of investment it may be helpful, therefore, to look at the evidence.

#### *E. Some Preliminary Evidence on the Basic Propositions*

Unfortunately the evidence which has been assembled so far is amazingly skimpy. Indeed, we have been able to locate only two recent studies—and these of rather limited scope—which were designed to throw light on the issue. Pending the results of more comprehensive tests which we hope will soon be available, we shall review briefly such evidence as is provided by the two studies in question: (1) an analysis of the relation between security yields and financial structure for some 43 large electric utilities by F. B. Allen [1], and (2) a parallel (unpublished) study by Robert Smith [19], for 42 oil companies designed to test whether Allen's rather striking results would be found in an industry with very different characteristics.<sup>35</sup> The Allen study is based on average figures for the years 1947 and 1948, while the Smith study relates to the single year 1953.

*The Effect of Leverage on the Cost of Capital.* According to the received view, as shown in equation (17) the average cost of capital,  $\bar{X}/V$ , should decline linearly with leverage as measured by the ratio  $D/V$ , at least through most of the relevant range.<sup>36</sup> According to Proposition I, the average cost of capital within a given class  $k$  should tend to have the same value  $\rho_k$  independently of the degree of leverage. A simple test

<sup>34</sup> Several specific examples of the failure of the arbitrage mechanism can be found in Graham and Dodd [6, e.g., pp. 646–48]. The price discrepancy described on pp. 646–47 is particularly curious since it persists even today despite the fact that a whole generation of security analysts has been brought up on this book!

<sup>35</sup> We wish to express our thanks to both writers for making available to us some of their original worksheets. In addition to these recent studies there is a frequently cited (but apparently seldom read) study by the Federal Communications Commission in 1938 [22] which purports to show the existence of an optimal capital structure or range of structures (in the sense defined above) for public utilities in the 1930's. By current standards for statistical investigations, however, this study cannot be regarded as having any real evidential value for the problem at hand.

<sup>36</sup> We shall simplify our notation in this section by dropping the subscript  $j$  used to denote a particular firm wherever this will not lead to confusion.



of the merits of the two alternative hypotheses can thus be carried out by correlating  $\bar{X}^*/V$  with  $D/V$ . If the traditional view is correct, the correlation should be significantly negative; if our view represents a better approximation to reality, then the correlation should not be significantly different from zero.

Both studies provide information about the average value of  $D$ —the market value of bonds and preferred stock—and of  $V$ —the market value of all securities.<sup>37</sup> From these data we can readily compute the ratio  $D/V$  and this ratio (expressed as a percentage) is represented by the symbol  $d$  in the regression equations below. The measurement of the variable  $\bar{X}^*/V$ , however, presents serious difficulties. Strictly speaking, the numerator should measure the expected returns net of taxes, but this is a variable on which no direct information is available. As an approximation, we have followed both authors and used (1) the average value of actual net returns in 1947 and 1948 for Allen's utilities; and (2) actual net returns in 1953 for Smith's oil companies. Net return is defined in both cases as the sum of interest, preferred dividends and stockholders' income net of corporate income taxes. Although this approximation to expected returns is undoubtedly very crude, there is no reason to believe that it will systematically bias the test in so far as the sign of the regression coefficient is concerned. The roughness of the approximation, however, will tend to make for a wide scatter. Also contributing to the scatter is the crudeness of the industrial classification, since especially within the sample of oil companies, the assumption that all the firms belong to the same class in our sense, is at best only approximately valid.

Denoting by  $x$  our approximation to  $\bar{X}^*/V$  (expressed, like  $d$ , as a percentage), the results of the tests are as follows:

$$\text{Electric Utilities } x = 5.3 + .006d \quad r = .12 \\ (\pm .008)$$

$$\text{Oil Companies } x = 8.5 + .006d \quad r = .04. \\ (\pm .024)$$

The data underlying these equations are also shown in scatter diagram form in Figures 3 and 4.

The results of these tests are clearly favorable to our hypothesis.

<sup>37</sup> Note that for purposes of this test preferred stocks, since they represent an *expected* fixed obligation, are properly classified with bonds even though the tax status of preferred dividends is different from that of interest payments and even though preferred dividends are really fixed only as to their maximum in any year. Some difficulty of classification does arise in the case of convertible preferred stocks (and convertible bonds) selling at a substantial premium, but fortunately very few such issues were involved for the companies included in the two studies. Smith included bank loans and certain other short-term obligations (at book values) in his data on oil company debts and this treatment is perhaps open to some question. However, the amounts involved were relatively small and check computations showed that their elimination would lead to only minor differences in the test results.

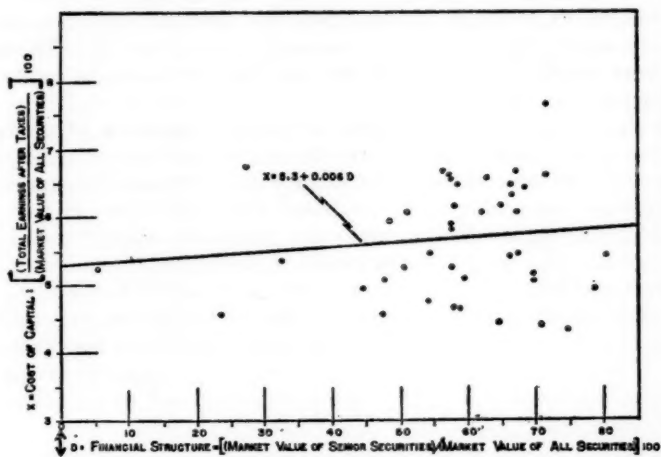


FIGURE 3. COST OF CAPITAL IN RELATION TO FINANCIAL STRUCTURE  
FOR 43 ELECTRIC UTILITIES, 1947-48

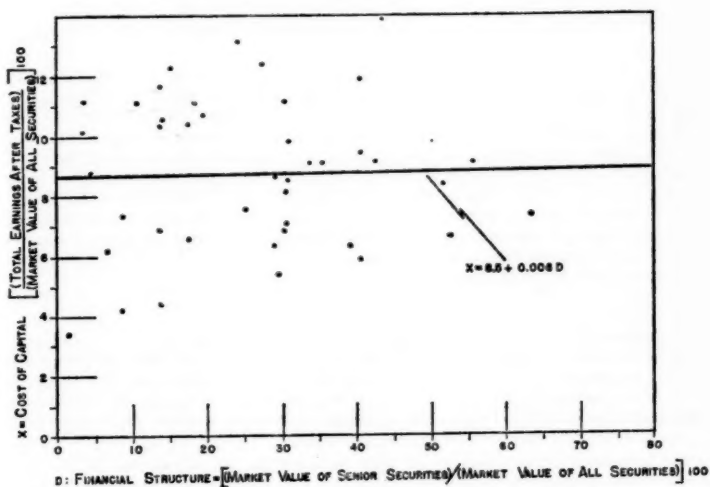


FIGURE 4. COST OF CAPITAL IN RELATION TO FINANCIAL STRUCTURE  
FOR 42 OIL COMPANIES, 1953

Both correlation coefficients are very close to zero and not statistically significant. Furthermore, the implications of the traditional view fail to be supported even with respect to the sign of the correlation. The data in short provide no evidence of any tendency for the cost of capital to fall as the debt ratio increases.<sup>28</sup>

It should also be apparent from the scatter diagrams that there is no hint of a curvilinear, U-shaped, relation of the kind which is widely believed to hold between the cost of capital and leverage. This graphical impression was confirmed by statistical tests which showed that for both industries the curvature was not significantly different from zero, its sign actually being opposite to that hypothesized.<sup>29</sup>

Note also that according to our model, the constant terms of the regression equations are measures of  $\rho_k^*$ , the capitalization rates for unlevered streams and hence the average cost of capital in the classes in question. The estimates of 8.5 per cent for the oil companies as against 5.3 per cent for electric utilities appear to accord well with a priori expectations, both in absolute value and relative spread.

*The Effect of Leverage on Common Stock Yields.* According to our Proposition II—see equation 12 and Figure 2—the expected yield on common stock,  $\bar{\pi}/S$ , in any given class, should tend to increase with leverage as measured by the ratio  $D/S$ . The relation should tend to be linear and with positive slope through most of the relevant range (as in the curve  $MM'$  of Figure 2), though it might tend to flatten out if we move

<sup>28</sup> It may be argued that a test of the kind used is biased against the traditional view. The fact that both sides of the regression equation are divided by the variable  $V$  which may be subject to random variation might tend to impart a positive bias to the correlation. As a check on the results presented in the text, we have, therefore, carried out a supplementary test based on equation (16). This equation shows that, if the traditional view is correct, the market value of a company should, for given  $\bar{X}^*$ , increase with debt through most of the relevant range; according to our model the market value should be uncorrelated with  $D$ , given  $\bar{X}^*$ . Because of wide variations in the size of the firms included in our samples, all variables must be divided by a suitable scale factor in order to avoid spurious results in carrying out a test of equation (16). The factor we have used is the book value of the firm denoted by  $A$ . The hypothesis tested thus takes the specific form:

$$V/A = a + b(\bar{X}^*/A) + c(D/A)$$

and the numerator of the ratio  $\bar{X}^*/A$  is again approximated by actual net returns. The partial correlation between  $V/A$  and  $D/A$  should now be positive according to the traditional view and zero according to our model. Although division by  $A$  should, if anything, bias the results in favor of the traditional hypothesis, the partial correlation turns out to be only .03 for the oil companies and -.28 for the electric utilities. Neither of these coefficients is significantly different from zero and the larger one even has the wrong sign.

<sup>29</sup> The tests consisted of fitting to the data the equation (19) of footnote 27. As shown there, it follows from the U-shaped hypothesis that the coefficient  $\alpha$  of the variable  $(D/V)^2/(1-D/V)$ , denoted hereafter by  $d^*$ , should be significant and positive. The following regression equations and partials were obtained:

$$\text{Electric Utilities } x = 5.0 + .017d - .003d^*; r_{x d^*} = -.15$$

$$\text{Oil Companies } x = 8.0 + .05d - .03d^*; r_{x d^*} = -.14$$

far enough to the right (as in the curve  $MD'$ ), to the extent that high leverage tends to drive up the cost of senior capital. According to the conventional view, the yield curve as a function of leverage should be a horizontal straight line (like  $ML'$ ) through most of the relevant range; far enough to the right, the yield may tend to rise at an increasing rate. Here again, a straight-forward correlation—in this case between  $\bar{\pi}/S$  and  $D/S$ —can provide a test of the two positions. If our view is correct, the correlation should be significantly positive; if the traditional view is correct, the correlation should be negligible.

Subject to the same qualifications noted above in connection with  $\bar{X}$ , we can approximate  $\bar{\pi}$  by actual stockholder net income.<sup>40</sup> Letting  $z$  denote in each case the approximation to  $\bar{\pi}/S$  (expressed as a percentage) and letting  $h$  denote the ratio  $D/S$  (also in percentage terms) the following results are obtained:

$$\text{Electric Utilities } z = 6.6 + .017h \quad r = .53 \\ (\pm .004)$$

$$\text{Oil Companies } z = 8.9 + .051h \quad r = .53. \\ (\pm .012)$$

These results are shown in scatter diagram form in Figures 5 and 6.

Here again the implications of our analysis seem to be borne out by the data. Both correlation coefficients are positive and highly significant when account is taken of the substantial sample size. Furthermore, the estimates of the coefficients of the equations seem to accord reasonably well with our hypothesis. According to equation (12) the constant term should be the value of  $\rho_k r$  for the given class while the slope should be  $(\rho_k r - r)$ . From the test of Proposition I we have seen that for the oil companies the mean value of  $\rho_k r$  could be estimated at around 8.7. Since the average yield of senior capital during the period covered was in the order of  $3\frac{1}{2}$  per cent, we should expect a constant term of about 8.7 per cent and a slope of just over 5 per cent. These values closely approximate the regression estimates of 8.9 per cent and 5.1 per cent respectively. For the electric utilities, the yield of senior capital was also on the order of  $3\frac{1}{2}$  per cent during the test years, but since the estimate of the mean value of  $\rho_k r$  from the test of Proposition I was 5.6 per cent,

<sup>40</sup> As indicated earlier, Smith's data were for the single year 1953. Since the use of a single year's profits as a measure of expected profits might be open to objection we collected profit data for 1952 for the same companies and based the computation of  $\bar{\pi}/S$  on the average of the two years. The value of  $\bar{\pi}/S$  was obtained from the formula:

$$\left( \frac{\text{net earnings in 1952} \cdot \frac{\text{assets in '53}}{\text{assets in '52}} + \text{net earnings in '1953}}{2} \right) \frac{1}{2} \\ \div (\text{average market value of common stock in '53}).$$

The asset adjustment was introduced as rough allowance for the effects of possible growth in the size of the firm. It might be added that the correlation computed with  $\bar{\pi}/S$  based on net profits in 1953 alone was found to be only slightly smaller, namely .50.

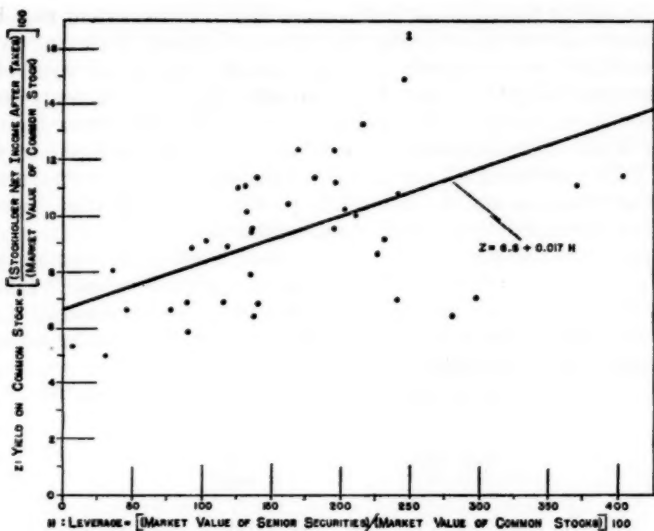


FIGURE 5. YIELD ON COMMON STOCK IN RELATION TO LEVERAGE FOR 43 ELECTRIC UTILITIES, 1947-48

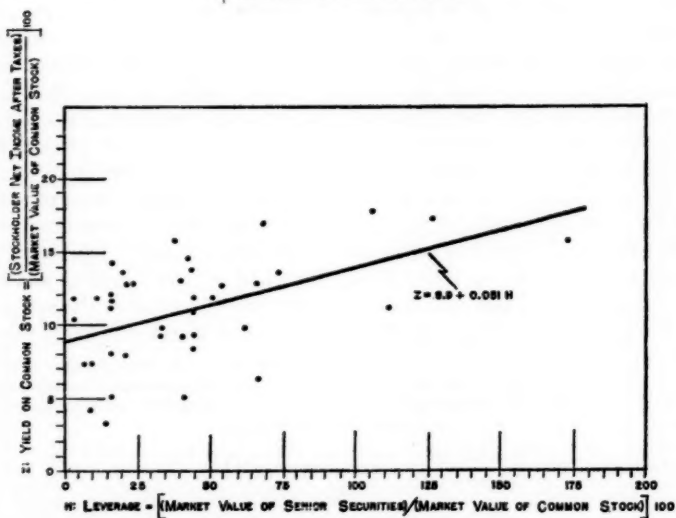


FIGURE 6. YIELD ON COMMON STOCK IN RELATION TO LEVERAGE FOR 42 OIL COMPANIES, 1952-53

the slope should be just above 2 per cent. The actual regression estimate for the slope of 1.7 per cent is thus somewhat low, but still within one standard error of its theoretical value. Because of this underestimate of the slope and because of the large mean value of leverage ( $\bar{h}=160$  per cent) the regression estimate of the constant term, 6.6 per cent, is somewhat high, although not significantly different from the value of 5.6 per cent obtained in the test of Proposition I.

When we add a square term to the above equations to test for the presence and direction of curvature we obtain the following estimates:

$$\text{Electric Utilities } z = 4.6 + .004h - .007h^2$$

$$\text{Oil Companies } z = 8.5 + .072h - .016h^2$$

For both cases the curvature is negative. In fact, for the electric utilities, where the observations cover a wider range of leverage ratios, the negative coefficient of the square term is actually significant at the 5 per cent level. Negative curvature, as we have seen, runs directly counter to the traditional hypothesis, whereas it can be readily accounted for by our model in terms of rising cost of borrowed funds.<sup>41</sup>

In summary, the empirical evidence we have reviewed seems to be broadly consistent with our model and largely inconsistent with traditional views. Needless to say much more extensive testing will be required before we can firmly conclude that our theory describes market behavior. Caution is indicated especially with regard to our test of Proposition II, partly because of possible statistical pitfalls<sup>42</sup> and partly because not all the factors that might have a systematic effect on stock yields have been considered. In particular, no attempt was made to test the possible influence of the dividend pay-out ratio whose role has tended to receive a great deal of attention in current research and thinking. There are two reasons for this omission. First, our main objective has been to assess the *prima facie* tenability of *our* model, and in this model, based as it is on rational behavior by investors, dividends *per se* play no role. Second, in a world in which the policy of dividend stabilization is widespread, there is no simple way of disentangling the true effect of dividend payments on stock prices from their apparent effect,

<sup>41</sup> That the yield of senior capital tended to rise for utilities as leverage increased is clearly shown in several of the scatter diagrams presented in the published version of Allen's study. This significant negative curvature between stock yields and leverage for utilities may be partly responsible for the fact, previously noted, that the constant in the linear regression is somewhat higher and the slope somewhat lower than implied by equation (12). Note also in connection with the estimate of  $\rho_s$  that the introduction of the quadratic term reduces the constant considerably, pushing it in fact below the *a priori* expectation of 5.6, though the difference is again not statistically significant.

<sup>42</sup> In our test, *e.g.*, the two variables  $z$  and  $h$  are both ratios with  $S$  appearing in the denominator, which may tend to impart a positive bias to the correlation (*cf.* note 38). Attempts were made to develop alternative tests, but although various possibilities were explored, we have so far been unable to find satisfactory alternatives.

the latter reflecting only the role of dividends as a proxy measure of long-term earning anticipations.<sup>43</sup> The difficulties just mentioned are further compounded by possible interrelations between dividend policy and leverage.<sup>44</sup>

## II. Implications of the Analysis for the Theory of Investment

### A. Capital Structure and Investment Policy

On the basis of our propositions with respect to cost of capital and financial structure (and for the moment neglecting taxes), we can derive the following simple rule for optimal investment policy by the firm:

*Proposition III.* If a firm in class  $k$  is acting in the best interest of the stockholders at the time of the decision, it will exploit an investment opportunity if and only if the rate of return on the investment, say  $\rho^*$ , is as large as or larger than  $\rho_k$ . That is, *the cut-off point for investment in the firm will in all cases be  $\rho_k$  and will be completely unaffected by the type of security used to finance the investment.* Equivalently, we may say that regardless of the financing used, the marginal cost of capital to a firm is equal to the average cost of capital, which is in turn equal to the capitalization rate for an unlevered stream in the class to which the firm belongs.<sup>45</sup>

To establish this result we will consider the three major financing alternatives open to the firm—bonds, retained earnings, and common stock issues—and show that in each case an investment is worth undertaking if, and only if,  $\rho^* \geq \rho_k$ .<sup>46</sup>

Consider first the case of an investment financed by the sale of bonds. We know from Proposition I that the market value of the firm before the investment was undertaken was:<sup>47</sup>

$$(20) \quad V_0 = \bar{X}_0 / \rho_k$$

<sup>43</sup> We suggest that failure to appreciate this difficulty is responsible for many fallacious, or at least unwarranted, conclusions about the role of dividends.

<sup>44</sup> In the sample of electric utilities, there is a substantial negative correlation between yields and pay-out ratios, but also between pay-out ratios and leverage, suggesting that either the association of yields and leverage or of yields and pay-out ratios may be (at least partly) spurious. These difficulties however do not arise in the case of the oil industry sample. A preliminary analysis indicates that there is here no significant relation between leverage and pay-out ratios and also no significant correlation (either gross or partial) between yields and pay-out ratios.

<sup>45</sup> The analysis developed in this paper is essentially a comparative-statics, not a dynamic analysis. This note of caution applies with special force to Proposition III. Such problems as those posed by expected changes in  $r$  and in  $\rho_k$  over time will not be treated here. Although they are in principle amenable to analysis within the general framework we have laid out, such an undertaking is sufficiently complex to deserve separate treatment. Cf. note 17.

<sup>46</sup> The extension of the proof to other types of financing, such as the sale of preferred stock or the issuance of stock rights is straightforward.

<sup>47</sup> Since no confusion is likely to arise, we have again, for simplicity, eliminated the subscripts identifying the firm in the equations to follow. Except for  $\rho_k$ , the subscripts now refer to time periods.



and that the value of the common stock was:

$$(21) \quad S_0 = V_0 - D_0.$$

If now the firm borrows  $I$  dollars to finance an investment yielding  $\rho^*$  its market value will become:

$$(22) \quad V_1 = \frac{\bar{X}_0 + \rho^* I}{\rho_k} = V_0 + \frac{\rho^* I}{\rho_k}$$

and the value of its common stock will be:

$$(23) \quad S_1 = V_1 - (D_0 + I) = V_0 + \frac{\rho^* I}{\rho_k} - D_0 - I$$

or using equation 21,

$$(24) \quad S_1 = S_0 + \frac{\rho^* I}{\rho_k} - I.$$

Hence  $S_1 \geq S_0$  as  $\rho^* \geq \rho_k$ .<sup>48</sup>

To illustrate, suppose the capitalization rate for uncertain streams in the  $k$ th class is 10 per cent and the rate of interest is 4 per cent. Then if a given company had an expected income of 1,000 and if it were financed entirely by common stock we know from Proposition I that the market value of its stock would be 10,000. Assume now that the managers of the firm discover an investment opportunity which will require an outlay of 100 and which is expected to yield 8 per cent. At first sight this might appear to be a profitable opportunity since the expected return is double the interest cost. If, however, the management borrows the necessary 100 at 4 per cent, the total expected income of the company rises to 1,008 and the market value of the firm to 10,080. But the firm now will have 100 of bonds in its capital structure so that, paradoxically, the market value of the stock must actually be reduced from 10,000 to 9,980 as a consequence of this apparently profitable investment. Or, to put it another way, the gains from being able to tap cheap, borrowed funds are more than offset for the stockholders by the market's discounting of the stock for the added leverage assumed.

Consider next the case of retained earnings. Suppose that in the course of its operations the firm acquired  $I$  dollars of cash (without impairing

<sup>48</sup> In the case of bond-financing the rate of interest on bonds does not enter explicitly into the decision (assuming the firm borrows at the market rate of interest). This is true, moreover, given the conditions outlined in Section I.C, even though interest rates may be an increasing function of debt outstanding. To the extent that the firm borrowed at a rate other than the market rate the two  $I$ 's in equation (24) would no longer be identical and an additional gain or loss, as the case might be, would accrue to the shareholders. It might also be noted in passing that permitting the two  $I$ 's in (24) to take on different values provides a simple method for introducing underwriting expenses into the analysis.

the earning power of its assets). If the cash is distributed as a dividend to the stockholders their wealth  $W_0$ , after the distribution will be:

$$(25) \quad W_0 = S_0 + I = \frac{\bar{X}_0}{\rho_k} - D_0 + I$$

where  $\bar{X}_0$  represents the expected return from the assets exclusive of the amount  $I$  in question. If however the funds are retained by the company and used to finance new assets whose expected rate of return is  $\rho^*$ , then the stockholders' wealth would become:

$$(26) \quad W_1 = S_1 = \frac{\bar{X}_0 + \rho^* I}{\rho_k} - D_0 = S_0 + \frac{\rho^* I}{\rho_k}.$$

Clearly  $W_1 \geq W_0$  as  $\rho^* \geq \rho_k$  so that an investment financed by retained earnings raises the net worth of the owners if and only if  $\rho^* > \rho_k$ .<sup>49</sup>

Consider finally, the case of common-stock financing. Let  $P_0$  denote the current market price per share of stock and assume, for simplicity, that this price reflects currently expected earnings only, that is, it does not reflect any future increase in earnings as a result of the investment under consideration.<sup>50</sup> Then if  $N$  is the original number of shares, the price per share is:

$$(27) \quad P_0 = S_0/N$$

and the number of new shares,  $M$ , needed to finance an investment of  $I$  dollars is given by:

$$(28) \quad M = \frac{I}{P_0}.$$

As a result of the investment the market value of the stock becomes:

$$S_1 = \frac{\bar{X}_0 + \rho^* I}{\rho_k} - D_0 = S_0 + \frac{\rho^* I}{\rho_k} = NP_0 + \frac{\rho^* I}{\rho_k}$$

and the price per share:

$$(29) \quad P_1 = \frac{S_1}{N + M} = \frac{1}{N + M} \left[ NP_0 + \frac{\rho^* I}{\rho_k} \right].$$

<sup>49</sup> The conclusion that  $\rho_k$  is the cut-off point for investments financed from internal funds applies not only to undistributed net profits, but to depreciation allowances (and even to the funds represented by the current sale value of any asset or collection of assets). Since the owners can earn  $\rho_k$  by investing funds elsewhere in the class, partial or total liquidating distributions should be made whenever the firm cannot achieve a marginal internal rate of return equal to  $\rho_k$ .

<sup>50</sup> If we assumed that the market price of the stock did reflect the expected higher future earnings (as would be the case if our original set of assumptions above were strictly followed) the analysis would differ slightly in detail, but not in essentials. The cut-off point for new investment would still be  $\rho_k$ , but where  $\rho^* > \rho_k$  the gain to the original owners would be larger than if the stock price were based on the pre-investment expectations only.

Since by equation (28),  $I = MP_0$ , we can add  $MP_0$  and subtract  $I$  from the quantity in bracket, obtaining:

$$(30) \quad \begin{aligned} P_1 &= \frac{1}{N+M} \left[ (N+M)P_0 + \frac{\rho^* - \rho_k}{\rho_k} I \right] \\ &= P_0 + \frac{1}{N+M} \frac{\rho^* - \rho_k}{\rho_k} I > P_0 \text{ if,} \end{aligned}$$

and only if,  $\rho^* > \rho_k$ .

Thus an investment financed by common stock is advantageous to the current stockholders if and only if its yield exceeds the capitalization rate  $\rho_k$ .

Once again a numerical example may help to illustrate the result and make it clear why the relevant cut-off rate is  $\rho_k$  and not the current yield on common stock,  $i$ . Suppose that  $\rho_k$  is 10 per cent,  $r$  is 4 per cent, that the original expected income of our company is 1,000 and that management has the opportunity of investing 100 having an expected yield of 12 per cent. If the original capital structure is 50 per cent debt and 50 per cent equity, and 1,000 shares of stock are initially outstanding, then, by Proposition I, the market value of the common stock must be 5,000 or 5 per share. Furthermore, since the interest bill is  $.04 \times 5,000 = 200$ , the yield on common stock is  $800/5,000 = 16$  per cent. It may then appear that financing the additional investment of 100 by issuing 20 shares to outsiders at 5 per share would dilute the equity of the original owners since the 100 promises to yield 12 per cent whereas the common stock is currently yielding 16 per cent. Actually, however, the income of the company would rise to 1,012; the value of the firm to 10,120; and the value of the common stock to 5,120. Since there are now 1,020 shares, each would be worth 5.02 and the wealth of the original stockholders would thus have been increased. What has happened is that the dilution in expected earnings per share (from .80 to .796) has been more than offset, in its effect upon the market price of the shares, by the decrease in leverage.

Our conclusion is, once again, at variance with conventional views,<sup>51</sup> so much so as to be easily misinterpreted. Read hastily, Proposition III seems to imply that the capital structure of a firm is a matter of indifference; and that, consequently, one of the core problems of corporate finance—the problem of the optimal capital structure for a firm—is no problem at all. It may be helpful, therefore, to clear up such possible misunderstandings.

<sup>51</sup> In the matter of investment policy under uncertainty there is no single position which represents "accepted" doctrine. For a sample of current formulations, all very different from ours, see Joel Dean [2, esp. Ch. 3], M. Gordon and E. Shapiro [5], and Harry Roberts [17].

*B. Proposition III and Financial Planning by Firms*

Misinterpretation of the scope of Proposition III can be avoided by remembering that this Proposition tells us only that the type of instrument used to finance an investment is irrelevant to the question of whether or not the investment is worth while. This does not mean that the owners (or the managers) have no grounds whatever for preferring one financing plan to another; or that there are no other policy or technical issues in finance at the level of the firm.

That grounds for preferring one type of financial structure to another will still exist within the framework of our model can readily be seen for the case of common-stock financing. In general, except for something like a widely publicized oil-strike, we would expect the market to place very heavy weight on current and recent past earnings in forming expectations as to future returns. Hence, if the owners of a firm discovered a major investment opportunity which they felt would yield much more than  $\rho_k$ , they might well prefer not to finance it via common stock at the then ruling price, because this price may fail to capitalize the new venture. A better course would be a pre-emptive issue of stock (and in this connection it should be remembered that stockholders are free to borrow and buy). Another possibility would be to finance the project initially with debt. Once the project had reflected itself in increased actual earnings, the debt could be retired either with an equity issue at much better prices or through retained earnings. Still another possibility along the same lines might be to combine the two steps by means of a convertible debenture or preferred stock, perhaps with a progressively declining conversion rate. Even such a double-stage financing plan may possibly be regarded as yielding too large a share to outsiders since the new stockholders are, in effect, being given an interest in any similar opportunities the firm may discover in the future. If there is a reasonable prospect that even larger opportunities may arise in the near future and if there is some danger that borrowing now would preclude more borrowing later, the owners might find their interests best protected by splitting off the current opportunity into a separate subsidiary with independent financing. Clearly the problems involved in making the crucial estimates and in planning the optimal financial strategy are by no means trivial, even though they should have no bearing on the basic decision to invest (as long as  $\rho^* \geq \rho_k$ ).<sup>62</sup>

Another reason why the alternatives in financial plans may not be a matter of indifference arises from the fact that managers are concerned

<sup>62</sup> Nor can we rule out the possibility that the existing owners, if unable to use a financing plan which protects their interest, may actually prefer to pass up an otherwise profitable venture rather than give outsiders an "excessive" share of the business. It is presumably in situations of this kind that we could justifiably speak of a shortage of "equity capital," though this kind of market imperfection is likely to be of significance only for small or new firms.

with more than simply furthering the interest of the owners. Such other objectives of the management—which need not be necessarily in conflict with those of the owners—are much more likely to be served by some types of financing arrangements than others. In many forms of borrowing agreements, for example, creditors are able to stipulate terms which the current management may regard as infringing on its prerogatives or restricting its freedom to maneuver. The creditors might even be able to insist on having a direct voice in the formation of policy.<sup>53</sup> To the extent, therefore, that financial policies have these implications for the management of the firm, something like the utility approach described in the introductory section becomes relevant to financial (as opposed to investment) decision-making. It is, however, the utility functions of the managers per se and not of the owners that are now involved.<sup>54</sup>

In summary, many of the specific considerations which bulk so large in traditional discussions of corporate finance can readily be superimposed on our simple framework without forcing any drastic (and certainly no systematic) alteration of the conclusion which is our principal concern, namely that for investment decisions, the marginal cost of capital is  $\rho_k$ .

### C. *The Effect of the Corporate Income Tax on Investment Decisions*

In Section I it was shown that when an unintegrated corporate income tax is introduced, the original version of our Proposition I,

$$\bar{X}/V = \rho_k = \text{a constant}$$

must be rewritten as:

$$(11) \quad \frac{(\bar{X} - rD)(1 - \tau) + rD}{V} = \frac{\bar{X}^*}{V} = \rho_k^* = \text{a constant.}$$

Throughout Section I we found it convenient to refer to  $\bar{X}/V$  as the cost of capital. The appropriate measure of the cost of capital relevant

<sup>53</sup> Similar considerations are involved in the matter of dividend policy. Even though the stockholders may be indifferent as to payout policy as long as investment policy is optimal, the management need not be so. Retained earnings involve far fewer threats to control than any of the alternative sources of funds and, of course, involve no underwriting expense or risk. But against these advantages management must balance the fact that sharp changes in dividend rates, which heavy reliance on retained earnings might imply, may give the impression that a firm's finances are being poorly managed, with consequent threats to the control and professional standing of the management.

<sup>54</sup> In principle, at least, this introduction of management's risk preferences with respect to financing methods would do much to reconcile the apparent conflict between Proposition III and such empirical findings as those of Modigliani and Zeman [14] on the close relation between interest rates and the ratio of new debt to new equity issues; or of John Lintner [12] on the considerable stability in target and actual dividend-payout ratios.

to investment decisions, however, is the ratio of the expected return *before* taxes to the market value, *i.e.*,  $\bar{X}/V$ . From (11) above we find:

$$(31) \quad \frac{\bar{X}}{V} = \frac{\rho_k^* - \tau_r(D/V)}{1 - \tau} = \frac{\rho_k^*}{1 - \tau} \left[ 1 - \frac{\tau r D}{\rho_k^* V} \right],$$

which shows that the cost of capital now depends on the debt ratio, decreasing, as  $D/V$  rises, at the constant rate  $\tau r/(1-\tau)$ .<sup>55</sup> Thus, with a corporate income tax under which interest is a deductible expense, gains can accrue to stockholders from having debt in the capital structure, even when capital markets are perfect. The gains however are small, as can be seen from (31), and as will be shown more explicitly below.

From (31) we can develop the tax-adjusted counterpart of Proposition III by interpreting the term  $D/V$  in that equation as the proportion of debt used in any additional financing of  $V$  dollars. For example, in the case where the financing is entirely by new common stock,  $D=0$  and the required rate of return  $\rho_k^S$  on a venture so financed becomes:

$$(32) \quad \rho_k^S = \frac{\rho_k^*}{1 - \tau}.$$

For the other extreme of pure debt financing  $D=V$  and the required rate of return,  $\rho_k^D$ , becomes:

$$(33) \quad \rho_k^D = \frac{\rho_k^*}{1 - \tau} \left[ 1 - \tau \frac{r}{\rho_k^*} \right] = \rho_k^S \left[ 1 - \tau \frac{r}{\rho_k^*} \right] = \rho_k^S - \frac{\tau}{1 - \tau} r. \quad ^{56}$$

For investments financed out of retained earnings, the problem of defining the required rate of return is more difficult since it involves a comparison of the tax consequences to the individual stockholder of receiving a dividend versus having a capital gain. Depending on the time of realization, a capital gain produced by retained earnings may be taxed either at ordinary income tax rates, 50 per cent of these rates, 25 per

<sup>55</sup> Equation (31) is amenable, in principle, to statistical tests similar to those described in Section I.E. However we have not made any systematic attempt to carry out such tests so far, because neither the Allen nor the Smith study provides the required information. Actually, Smith's data included a very crude estimate of tax liability, and, using this estimate, we did in fact obtain a negative relation between  $\bar{X}/V$  and  $D/V$ . However, the correlation ( $-.28$ ) turned out to be significant only at about the 10 per cent level. While this result is not conclusive, it should be remembered that, according to our theory, the slope of the regression equation should be in any event quite small. In fact, with a value of  $\tau$  in the order of .5, and values of  $\rho_k^*$  and  $r$  in the order of 8.5 and 3.5 per cent respectively (*cf.* Section I.E) an increase in  $D/V$  from 0 to 60 per cent (which is, approximately, the range of variation of this variable in the sample) should tend to reduce the average cost of capital only from about 17 to about 15 per cent.

<sup>56</sup> This conclusion does not extend to preferred stocks even though they have been classed with debt issues previously. Since preferred dividends except for a portion of those of public utilities are not in general deductible from the corporate tax, the cut-off point for new financing via preferred stock is exactly the same as that for common stock.

cent, or zero, if held till death. The rate on any dividends received in the event of a distribution will also be a variable depending on the amount of other income received by the stockholder, and with the added complications introduced by the current dividend-credit provisions. If we assume that the managers proceed on the basis of reasonable estimates as to the average values of the relevant tax rates for the owners, then the required return for retained earnings  $\rho_k^R$  can be shown to be:

$$(34) \quad \rho_k^R = \rho_k^T \frac{1}{1 - \tau} \frac{1 - \tau_d}{1 - \tau_g} = \frac{1 - \tau_d}{1 - \tau_g} \rho_k^S$$

where  $\tau_d$  is the assumed rate of personal income tax on dividends and  $\tau_g$  is the assumed rate of tax on capital gains.

A numerical illustration may perhaps be helpful in clarifying the relationship between these required rates of return. If we take the following round numbers as representative order-of-magnitude values under present conditions: an after-tax capitalization rate  $\rho_k^T$  of 10 per cent, a rate of interest on bonds of 4 per cent, a corporate tax rate of 50 per cent, a marginal personal income tax rate on dividends of 40 per cent (corresponding to an income of about \$25,000 on a joint return), and a capital gains rate of 20 per cent (one-half the marginal rate on dividends), then the required rates of return would be: (1) 20 per cent for investments financed entirely by issuance of new common shares; (2) 16 per cent for investments financed entirely by new debt; and (3) 15 per cent for investments financed wholly from internal funds.

These results would seem to have considerable significance for current discussions of the effect of the corporate income tax on financial policy and on investment. Although we cannot explore the implications of the results in any detail here, we should at least like to call attention to the remarkably small difference between the "cost" of equity funds and debt funds. With the numerical values assumed, equity money turned out to be only 25 per cent more expensive than debt money, rather than something on the order of 5 times as expensive as is commonly supposed to be the case.<sup>67</sup> The reason for the wide difference is that the traditional

<sup>67</sup> See e.g., D. T. Smith [18]. It should also be pointed out that our tax system acts in other ways to reduce the gains from debt financing. Heavy reliance on debt in the capital structure, for example, commits a company to paying out a substantial proportion of its income in the form of interest payments taxable to the owners under the personal income tax. A debt-free company, by contrast, can reinvest in the business all of its (smaller) net income and to this extent subject the owners only to the low capital gains rate (or possibly no tax at all by virtue of the loophole at death). Thus, we should expect a high degree of leverage to be of value to the owners, even in the case of closely held corporations, primarily in cases where their firm was not expected to have much need for additional funds to expand assets and earnings in the future. To the extent that opportunities for growth were available, as they presumably would be for most successful corporations, the interest of the stockholders would tend to be better served by a structure which permitted maximum use of retained earnings.



view starts from the position that debt funds are several times cheaper than equity funds even in the absence of taxes, with taxes serving simply to magnify the cost ratio in proportion to the corporate rate. By contrast, in our model in which the repercussions of debt financing on the value of shares are taken into account, the *only* difference in cost is that due to the tax effect, and its magnitude is simply the tax on the "grossed up" interest payment. Not only is this magnitude likely to be small but our analysis yields the further paradoxical implication that the stockholders' gain from, and hence incentive to use, debt financing is actually smaller the lower the rate of interest. In the extreme case where the firm could borrow for practically nothing, the advantage of debt financing would also be practically nothing.

### III. Conclusion

With the development of Proposition III the main objectives we outlined in our introductory discussion have been reached. We have in our Propositions I and II at least the foundations of a theory of the valuation of firms and shares in a world of uncertainty. We have shown, moreover, how this theory can lead to an operational definition of the cost of capital and how that concept can be used in turn as a basis for rational investment decision-making within the firm. Needless to say, however, much remains to be done before the cost of capital can be put away on the shelf among the solved problems. Our approach has been that of static, partial equilibrium analysis. It has assumed among other things a state of atomistic competition in the capital markets and an ease of access to those markets which only a relatively small (though important) group of firms even come close to possessing. These and other drastic simplifications have been necessary in order to come to grips with the problem at all. Having served their purpose they can now be relaxed in the direction of greater realism and relevance, a task in which we hope others interested in this area will wish to share.

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## POLICY ANALYSIS

By CHARLES E. LINDBLOM\*

Where quantitative methods in economics are explicit, many other analytical methods are not. And lacking explicit statement, some are not even recognized to be distinctive methods. To be sure, everyone is familiar with paradigms of the scientific method, especially those describing the construction and use of theory; and of late increasing attention has been given to concept formation.<sup>1</sup> But many kinds of analysis still remain to be formalized, among which public policy analysis is one.

In seeking to formalize some of our methods of policy analysis, this paper is one of a growing family of ventures into clarification of non-quantitative and largely nontheoretical methods.<sup>2</sup> One noteworthy characteristic of these studies is that they are not argumentative: they do not urge this or that method upon social scientists; they merely make explicit and formalize the methods already in use.

In the analysis of public policy, economists will quickly recognize the following three (among many) characteristics of common procedures:

(1) A body of theory is applied to the particular situation in which a policy problem arises. (2) A comprehensive overview of all important variables is attempted by the analyst; or, if any important variable is neglected in the theory, the results are qualified to take it into account, or the user of the results is warned. (3) Postulated values specify criteria by which alternative policies are to be judged; or, if they are not simply postulated, their derivation is in any case a process separate from the purely scientific or positive analysis of variables. A fourth

\*The author is associate professor of economics at Yale University. To R. A. Dahl, William Fellner and Thomas Schelling, his thanks for helpful criticism and substantive suggestions. And for stimulating discussion from which some of the ideas in this paper arose, his thanks to colleagues at the first summer session of 1956 of the Merrill Center.

<sup>1</sup>For example, C. G. Hempel, "Fundamentals of Concept Formation in Empirical Science," *International Encyclopedia of Unified Science*, Vol. II, No. 7, Chicago 1952.

<sup>2</sup>Related studies include, among others that might be mentioned: P. F. Lazarsfeld and A. H. Barton, "Qualitative Measurement in the Social Sciences," in Daniel Lerner and H. D. Lasswell, eds., *The Policy Sciences*, Stanford 1951, Ch. 9; P. F. Lazarsfeld and R. K. Merton, "Friendship as Social Process: A Substantive and Methodological Analysis," in Monroe Berger, Theodore Abel, and C. H. Page, eds., *Freedom and Control in Modern Society*, New York 1954, Ch. 2; and Abraham Kaplan, "Definition and Specification of Meaning," *Jour. Philosophy*, May 23, 1946, XLIII, 281-88.

characteristic, though less frequently encountered, is nevertheless also common: (4) A presumption in favor of a particular kind of policy in a particular problem situation is derived from the general theoretical argument for that kind of policy treated as a general rule. For example, the general case for the price system and against direct controls creates, for those who accept it, a presumption (but only a presumption) for the use of the price system in particular policy-problem areas; or the general case for "planning," for those who accept it, creates a presumption for "planning" in a particular situation.

It should be noted that the first two listed refer to the handling of empirical material; the last two, to the handling of values. For simplicity, let me refer to these four procedures taken together as the "conventional" method of policy analysis, without implying anything about its correctness, frequency of use or academic respectability.

Now contrast to this familiar method a second which departs from—even negates—the first on each of the four characteristics. In the second method is found: (1) Relatively less reliance on theory. (2) A partial or fragmented view of the important variables. (3) A close intertwining of the search for values and the search for facts. (4) No policy presumption of the kind employed in the conventional method. In frequency of use, the second is quite as commonplace as the first and in its proper place no less accurate; but it cannot be described in the conventional terms we employ to describe scientific work. It is all the more interesting as a method because our first impulse is to look askance at it, believing that the merit of each of the four characteristics of the conventional method cannot be denied.

The second method is the subject of this paper. I shall try to make its processes explicit and display its contrasts with the conventional method by considering in turn each of the four points of difference.

### *I. Theory and Alternatives to It*

Just how the second method can and does proceed with a minimum of theory, and just what the second method consists of are best explained by reference to the limitations of theory, even at the risk of appearing to digress. For all the richness of economic theory, we realize that it embraces a limited number of variables and contains propositions about a limited number of possible situations. Much of our analysis carries us far beyond theory, and for some of our analysis theory is largely irrelevant. By observing our own practices, we know this to be true; and the point would not be worth dwelling on were it not for our common tendency to speak of theory and analysis as synonymous, as when a reader of an earlier draft of this paper claimed that analysis is "just another word for the use of theory."

To avoid quibbling over words, let me say that we often work out problems, organize our thoughts, find answers to questions, and come to conclusions without the aid of theory; and for that matter in most of the problem areas of the social sciences, no theory exists. In these senses, analysis and theory are not identical (and it is reasonable to ask what methods we employ for analysis other than theory).

Quite aside from *evaluation* of alternative policies, to which we turn later, the mere prediction or estimate of empirical consequences of alternative policies is often beyond our theoretical competence. Many of us would believe, for example, that while we could employ theory to predict that an economy without money and prices would in certain specified ways misallocate resources, we cannot rely heavily on theory in predicting that recent business mergers will or will not increase the extent of resource misallocation. While we rely on theoretical analysis to lead us to the conclusion that wartime government spending will produce inflation, it helps us only part way when we try to determine the relative merits of direct and indirect anti-inflation controls, as the extent of disagreement among economists and the content of their debate prove. In citing these examples, I am not trying to attack theory or pick a quarrel with it; I am only pointing out, as a basis for further clarification of method, something I assume we all agree on: specifically, that we commonly go in analysis where theory cannot take us. Something else is often, perhaps always, required.

Now one reason that we are forced to analyze variables outside the embrace of theory and establish propositions that cannot be found within our body of theory is that policy alternatives sometimes differ by degrees too fine for our theory to distinguish. If with theory one can contrast consequences of perfect and imperfect competition, one is nevertheless often unable to predict the different consequences of several alternative forms of competition all of which are imperfect. Yet policy must choose among forms of imperfect competition. Theory is often competent on the gross difference, but not on the more subtle ones.

Why do alternative public policies often differ only slightly? It is because our political procedures are what might be called incremental. Just what is embraced in the idea of incremental politics requires explanation because this kind of politics is central to the analytical method to be clarified. In incremental politics:

1. Political parties and leaders compete for votes by agreeing on fundamentals and offering only incrementally different policies in each of the policy areas in which they wish to compete. Since this has been well and frequently demonstrated to be a prerequisite condition for the

survival of democracy itself,<sup>3</sup> it is hardly to be questioned as a characteristic of political life in the Western democracies.

2. Each of the competing political parties shifts its own policies only incrementally at any one time. Such incremental alteration of party policies is in fact the normal though not invariable rule in all two-party democracies and in some multiparty democracies.<sup>4</sup>

3. Policy-making proceeds through a sequence of approximations. A policy is directed at a problem; it is tried, altered, tried in its altered form, altered again and so on. In short, incremental policies follow one upon the other in the solution to a given problem.

These are easily recognizable fundamental processes in American democracy and indeed in most if not all of the stable, well-established, deeply rooted democracies of the world. To be sure, ideological rhetoric pervades political debate and runs through general statements of purpose in legislative enactments; but that incremental politics dominates is evident in the manner in which the legislators themselves, as well as administrators and judges, incrementally implement the ideologically described purposes.

Why is a political body incrementalist? One set of conditions leading to the regular and persistent use of political incrementalism is the attitudes, interests and values that support political incrementalism. It would carry us too far afield to examine the incremental attitude in detail, but it requires brief elaboration. It is to be contrasted with the disposition to approach policy as though great ideological issues were to be decided. Its hold on the citizens of the most stable democracies is attributable to many factors, among which I mention three: (a) widespread consensus on fundamental values, (b) frequent widespread agreement on the general direction and character of desired social change, (c) relatively greater confidence in the predictability of consequences of incremental as against drastic social change. When an individual lives in a society marked by the kinds of consensus referred to and when he is dubious about the predictability of large-scale change, his demands upon political leadership and his response to their appeals to him will have the effect of buttressing incremental policies.

If then—to return to the main line of exposition—theory is sometimes inadequate and if its inadequacies are sometimes attributable to incremental politics, what alternative analytical method do we sometimes substitute? The second method rests on incremental politics and incremental attitudes and might be called incremental analysis.

<sup>3</sup> A more exact statement of the condition and an explanation of its necessity to democracy will be found in R. A. Dahl and C. E. Lindblom, *Politics, Economics and Welfare*, New York 1953, pp. 294ff.

<sup>4</sup> *Loc. cit.*

The method is *incremental* in a specific sense. When we practice it, we forswear the analysis of any large institutional complex as is often attempted with theory and attempt instead to predict the consequences of a specific hypothetical incremental alteration in public policy. We isolate the effects of the incremental change by examining differences in result between the *status quo* and each of several possible new situations that could be produced by a small change in public policy. Theory can be used incrementally too, but we shall see how this method becomes an alternative to the application of theory.

The method is one of *comparisons*. We compare results of several different possible policies, comparing each also with the policy of no change at all. The comparisons are chronologically *successive*. A succession of actual incremental policy alterations over a long period of time permits repeated comparisons among similar groups of incremental alterations in policy.

Whether we consider a hypothetical policy alternative to be incremental apparently depends upon: (a) the number of consequential variables that would be affected, and (b) the magnitude of the effect on each. A choice is less or more incremental on a continuum; at the incremental extreme is a choice that, with respect to a group of variables, affects only one and affects that one by the smallest amount that is still consequential. (But if, at the extreme, an incremental comparison is necessarily limited to one variable, in most actual cases, as will be later explained, we deliberately limit the number of variables by neglecting some of them.)

A policy will affect many or few variables depending upon what they are specified to be. And whether the variables are consequential, or the magnitude of the changes in them consequential, will depend upon how consequences are evaluated. For any analyst, therefore, a policy is incremental if, given the variables he considers consequential to his analysis and given his evaluations, the policy affects few of them and alters them only by small magnitudes. Clearly a choice can be incremental with respect to some values and nonincremental with respect to others, as well as incremental to one analyst and nonincremental to another.

The occasional eccentric who proposes to wipe the slate clean and begin again with a "new" human society and civilization is, of course, not an incremental analyst. Nor is the socialist, nor the doctrinaire nineteenth-century liberal in the twentieth century, nor the advocate of a centrally planned economy. On a continuum, the segment of policies to be described as incremental are such proposals as—and even these examples show considerable range themselves—lowering reserve requirements for banks, changing the retirement age under Old Age and Survivors' Insurance, requiring business notification of intention to merge



to the Federal Trade Commission, increasing military expenditures by 50 per cent, repealing the Taft-Hartley Act, establishing a Missouri Valley Authority, or removing certain agricultural subsidies or price supports.

It follows from what was said about the difficulties of theory and their relation to politics that our success with successive incremental comparisons will depend in largest part on whether political policy-making does in actual fact proceed through a sequence of what we consider to be incremental choices. In short, where politics is incremental, successive incremental comparisons help to make analysis feasible in the absence of adequate theory. But just what specific characteristics of incremental analysis do we find most significant for its capacity to assist us in the absence of adequate theory?

1. We find that the method sharply reduces our need for either a wide-ranging body of empirical generalizations or the propositions of a large formal theoretical system. We do not need propositions that describe a large institutional complex like competition because we limit our analysis to changes embracing only a few variables. Not that what theory we possess will be useless—it will often be a useful guideline or assist us in other ways—but it will not be essential.

2. We find that it similarly reduces our need for high-level generalizations on the fewer variables we do encounter. Specifically, we do not require relationships that hold over a large domain because we can be satisfied with understanding the relationship for the restricted domain within which our variables change in a particular policy choice. We can often so greatly reduce our need for generality in propositions as to make us independent of any that would be dignified by the name of scientific generalization.

The contrast on this point between incremental and conventional analysis as traditionally described is clear. Traditionally, we cannot proceed with a particular applied problem until we have found a generalization or group of generalizations that applies to our case. We must subsume our problems under a more general one. This means that behind our work lie many observations and derivations of generalizations from them, from which we now select what is appropriate to the particular instance. In incremental analysis the prior observations and derived generalizations are not always required, for we proceed directly to ascertaining the relationships among the few relevant variables in the particular situation that concerns us without troubling ourselves with whether there is a more general set of relations under which ours can be subsumed.

Presumably theories—and even scattered generalizations—are capital assets. To be able to do without them is not necessarily a virtue.

I do not deny this; I am simply clarifying a process for analysis in the absence of adequate theory or empirical generalizations.

3. From another point of view, we find that incremental analysis is a method for reducing the number and complexity of relations among variables to be considered. In predicting the consequences of incremental change only, it both reduces the required amount of information and at the same time limits problems to those within the grasp of the human intellect. More ambitious analysis very commonly embraces so many variables—demanding such quantities of information and such heroic organization of it—that the variables can only be treated implicitly; and neither the analyst nor his audience can be confident that they have been adequately considered.

Incremental analysis as a form of marginalism in analysis can be compared with consumer choice as another kind of marginalism in behavior. It is a commonplace that consumers make the problem of alternative product mixes manageable by ignoring all aspects of alternative mixes except the increment by which they differ. In the same way, incremental analysis simplifies the problem of alternative institutional mixes.

4. The product of our incremental analysis is a proposition stated in a form that often permits testing. Our outputs are fairly specific statements about policy that are either implicitly or explicitly in the form of "if this, then that." For example, if a soil bank program is enacted, then certain predicted consequences will follow. Where actual policy is altered by increments like this, such a modest predictive statement is often testable in practice. Hence, incremental analysis is to a degree self-corrective; and, moreover, sequences of tests through policy choices permit analysts to improve their skill in prediction.

5. The sequence of policy choices made up to any given time offers us a useful standard of relevance for choosing new incremental alterations to analyze. The number of possible alternative institutions or policies is beyond counting, hence beyond the funds, time, and manpower available to social science. Some principle of selection is in fact necessary, and we often find past sequences helpful.

## II. *Comprehensiveness and Fragmentation of Analysis*

The second point of difference between conventional and incremental analysis is the comprehensiveness of the former and the fragmentary quality of the latter. To the three essential elements of the incremental method, identified in the term "successive incremental comparisons," we add a fourth element: the comparisons are severely limited; they exclude certain important variables.

In considering why certain variables are ignored, it ought to be

remembered that I am describing not an idealized method but one practiced by fallible scholars. It follows that one reason that important variables are ignored is that the analyst unwittingly misses them. A second possibility is that certain policy consequences that in any one study are trivial—and are therefore ignored—become cumulatively significant through a succession of policy alterations. But a third possibility is that the analyst deliberately ignores what he knows to be an important variable.

Now as long as scholars look upon these omissions simply as aberrations, they will fail to understand that systematic omission is a method of analysis rather than a failure of method. But often omission is methodical. The important sense in which limitation or omission is part of a method and not a failure of method turns on the relation between analysis, on one hand, and the social processes in which analysis takes place, on the other. This point is critical and requires development.

The usefulness of an analytical method cannot be understood in isolation from the social processes through which it is applied. Limited analysis is suited to certain social processes in which analysis takes place. Consider a situation in which analysis of policy issues is widely dispersed among many different analysts representing many different scholarly interests, as in the United States. When analysis is thus "fragmented," limitation or omission in any one piece of analysis may actually be desirable. For while fragmentation may result in much lost motion and may sometimes undercut the cumulation of scientific knowledge that a centrally coordinated group of scholars might accomplish, it is also a process or method through which the successive limited incremental comparisons of many different scholars are coordinated, not in the sense that each knows what the other is doing but in the sense that each piece of analysis is nevertheless adjusted to each other.

Just as prices serve as parameters to the individual consumer solving his own consumption-behavior equations, so a state of affairs being analyzed by any one analyst is sometimes taken as a parameter, if not a given, by other analysts. More generally, what one analyst neglects, because his analysis is limited, another analyst sometimes makes central to his work. By analogy, in price-system behavior, an optimizing process of sorts can be shown. For fragmented analysis, any possibility of demonstrating an optimizing process, in which all relevant variables are adequately investigated by some analysts and taken account of as givens or parameters by all others, hinges upon what I shall call fragmentation of policy-making.

By fragmentation of policy-making, I mean to denote a political situation in which (a) there are more than one or a few policy-making

groups in the government, and (b) they display many different points of view as well as specialized functions. This is clearly the case in all large governments, even in those most rigidly hierarchical; and fragmentation can go to extremes if the governmental process is little more than warfare between interest groups.

For several reasons too obvious to mention, fragmentation of policy-making may go so far as to be undesirable, even taking account of the variety of standards by which it might be judged. And it may, other objections to it aside, make prediction in policy analysis more difficult. But one should not take these possible evils for granted; and, in order to throw light on possible uses of limited and fragmented analysis, I should like to develop a line of argument displaying possible fruitful interconnections between it on the one hand, and fragmentation of policy-making on the other.

To begin with, fragmentation of policy-making as in the United States, probably encourages fragmentation of analysis. Many different centers of decision-making marked by many different interests and attitudes stimulate a diversity of analytical attacks on policy problems, possibly achieving therefore the advantage of analytical fragmentation. Whether fragmentation in politics induces fragmentation in analysis or not, it is in any case helpful to the successful employment of limited and fragmented analysis in two ways. First, when policy functions are dispersed among many different specialized groups, any one line of policy is more likely to be pursued through a succession of incremental policies. The group specialized to it has both time and motivation. Even where all group policies must be formally funneled through a hierarchically superior group, fragmentation places the initiation, intimate consideration, and execution of increments of policy in the hands of those who will carry on with a sequence. Hence, there will be many opportunities for analytical reconsideration when variables have been neglected.

Secondly, and more important, fragmentation in politics is helpful because the different points of view taken by the different groups in government serve to make each group something of a watchdog for certain variables against others. Now whatever the merits of this phenomenon generally, its importance to limited incremental analysis is enormous where in the analysis of any one policy problem certain variables are neglected. Fragmentation in politics, like analytical fragmentation, means that variables peripheral to one policy group become central to some others; hence variables peripheral to one analyst become central to another.

At best in any large government a large proportion of policy decisions—probably most of them—are attempts to correct the mistakes of previous policies. With political fragmentation, policy adjustments can be

rapid and persistent; and out of the somewhat specialized points of view of the decentralized policy groups a process can sometimes be generated in which a wide variety of variables are taken into account even if in every single policy problem taken alone some important variables are disregarded.

At their idealized best the relations between incremental analysis and political fragmentation constitute a method for the mutual adjustment of elements of policy to each other in the absence of sufficient human skill in both calculation and control to permit a single complex policy decision to achieve coordination. Neither scholar nor policy-maker has intelligence and accumulated information—and both are necessary—sufficient to permit an overview of the interrelationships among the variables of public policy for making one integrated economic policy decision or even sometimes a decision in a limited field of economic policy. Coordination or integration of policy is then to be achieved sometimes only through countless small adjustments of policy. It should be easy to understand the mutual adjustment of small policies in this fashion, since it is comparable—to turn again to the familiar analogy—to the mutual adjustment of prices through fragmented decisions where no over-all central simultaneous solution of the equations of the price system is attempted.

But, again, where the price system can produce a kind of optimum, it is not clear that fragmentation of policy-making can ever do so; and it is clear that in many circumstances it does not, for it can degenerate into an undesirable kind of minorites rule. However, the optimum achieved through the price system is a very limited one; hence, to demonstrate a comparable optimizing process in fragmentation of policy-making is perhaps a less ambitious task than first appears. In the works of a variety of political scientists a beginning has been made; they provide a description of a very rough and somewhat capricious optimizing process.<sup>5</sup>

Optimization or not, however, our point is that severe limitation of analysis, characteristic of incremental analysis, is a method adapted to a situation in which analysis is fragmented, and fragmentation of analysis is in turn abetted by political fragmentation.

### III. Values

The third respect in which incremental analysis differs from what I have arbitrarily called the conventional is in the method in which

<sup>5</sup> A. F. Bentley, *The Process of Government*, Chicago 1908; Pendleton Herring, *The Politics of Democracy*, New York 1940; John Dickinson, "Democratic Realities and Democratic Dogma," *Am. Pol. Sci. Rev.*, May 1930, XXIV, 283-309; David Truman, *The Governmental Process*, New York 1951; Earl Latham, *The Group Basis of Politics*, Ithaca 1952. See also C. E. Lindblom, *Bargaining: The Hidden Hand in Government*, U. S. Air Force Project RAND Research Memo. RM-1434-RC, Santa Monica 1955.

values are handled. Conventionally it is required that the analyst postulate a social welfare function or, lacking that, at least determine his own utility surfaces on the assumption that he treat them as relevant for the problem at hand. Policy choices are then made to maximize the welfare function.<sup>6</sup> More informally, the view is often put in the form of such a question as: "I really don't see how it is possible to take a position with respect to proposed policy alternatives if one does not have in mind a version of some good society and select a solution 'most' in accord with that vision."

Actually we find we often cannot postulate a welfare function, cannot describe our good society, or cannot with any confidence employ a picture of a good society in the appraisal of alternatives. First, we often lack a general formula or agreed procedure for aggregating individual welfare functions into a social welfare function. Second, even more important, the preferences or values of the individuals with whom we are concerned are often not known either to us or to the individuals themselves except through actual policy choices made. Individuals often learn and express their preferences through the sequence of policy choices they actually make rather than deduce the appropriate choice from a preknowledge of their preferences. Third, while many individuals are satisfied that they know their values in a general way in advance of choice situations, the relevant valuations are shifting marginal valuations; and these they do not know and ordinarily cannot know except when faced with an actual choice.

What we economists often actually do, therefore, is make successive limited incremental comparisons of ends or values as well as of means in policy analysis. The analyst who employs incremental analysis obtains the required information on his own or some group's values by observing his own or some group's choices made in the recent past. Because in incremental analysis he observes and studies incremental choice and because he excuses himself from acquiring competence on any values except the temporary marginal evaluations of relatively few variables, the requirement upon him that he comprehend the values relevant to his policy analysis is not an impossible one. And because his data on values are the data of actual choices that have been made in previous policy decisions, they will often be superior to any other value data to which he could turn.

Moreover, it is a characteristic of democratic societies that policies are not even ideally to be derived logically from a unified set of values, as could conceivably govern the policy-making of a dictatorship. In actual fact, it is common for citizens or their political leaders to agree on policies without their even raising the question of whether they agree

<sup>6</sup> See, as one of many possible examples, J. Tinbergen, *On The Theory of Economic Policy*, Amsterdam 1952, pp. 1-5.



on ultimate values, it being possible to agree on the former despite different values. Hence general postulated values are irrelevant to many policy choices. The analyst employing incremental analysis simplifies his analytical problem by avoiding the assumption that there is always, in addition to conflict on policy preferences, an additional conflict on more ultimate values with which he must come to grips.

The difference in the treatment of values in incremental and conventional analyses can be illustrated. In incremental analysis, the analyst realizes that marginal preferences among values are constantly shifting with different degrees of achievement of each value. Where in nonincremental analysis he may conceive of a group or of himself as, say, to a degree equalitarian, in incremental analysis he explores through a succession of policies the attitudes of the population (or of himself) toward more or less equality in a variety of fields, is unable to generalize widely on public attitudes (or his own attitude) toward equality, and is sensitive to the changing priorities of equality goals in different areas and to changing priorities in the whole list of goals, of which equality is only one. He knows that a population (or he himself) can value certain incremental alterations in policy in the direction of equality of income, without necessarily being disposed either toward or against equalization of income as a general rule, because a general rule is not very meaningful. In incremental analysis, values are not simply formulae for the guidance of policy choices but interact with policy choices in such a way that each unendingly alters the other as more is learned about both values and appropriate policies from the results of each incremental alteration in policy.

Again, the usefulness of the method depends upon the character of the society in which it is practiced. The incremental approach to values is best suited to a society in which values are incrementally explored and adjusted by the citizenry or by whatever part of the population whose interests concern the analyst. At an extreme, each individual in the society makes successive limited incremental comparisons of his own values. Hence, again as earlier, incremental analysis rests on incremental attitudes and politics.

#### *IV. Policy Presumptions*

The last differentiating characteristic of incremental analysis is that it avoids the kind of policy presumption that is found in conventional analysis. I can illustrate the conventional type of presumption by a set of ideas like the following: The price system, it is argued, is a highly specialized or differentiated social mechanism for economizing. It possesses fairly obvious merits for meeting most of the tasks of economic organization. Hence, although it is deficient in certain identifiable respects, one should approach a judgment on a policy question with



a presumption in favor of the price system.

This is a common procedure. For example, after postulating certain value objectives for monetary policy, Friedman writes: "I believe . . . that all three objectives can best be realized by relying, as far as possible, on a market mechanism within a 'competitive order' to organize the utilization of economic resources."<sup>7</sup>

Now since no mechanism is to be pressed "as far as possible" unless possible means desirable (in which case the statement is empty), Friedman's statement has to be read as establishing a presumption. But a general case for the competitive system to which he presumably appeals is clearly not *logically* sufficient to create a presumption for its use in any particular area in which a policy problem arises. Often, therefore, we will avoid such a presumption. In incremental analysis we will simply examine alternative incremental adjustments, our only presumption being that a policy is best developed if at any one time it is considered and instigated as an incremental adjustment of an otherwise unquestioned and unaltered institutional complex. Toward this institutional complex we bring no presumption, favorable or unfavorable, because for our analytical task it is simply a given.<sup>8</sup>

Furthermore, while terms like "competition," "capitalism," "planning" and "socialism" continue to be useful for historical writing and for certain other purposes, we recognize that in a politically incremental society they do not describe actual policy alternatives. Since all relevant real-world systems are combinations of these "systems," what is at stake at any point of marginal policy adjustment is a "systems-mix." Hence the general case for or against any of these pure systems, we find, is not a case for or against any actual policy choice that is in fact open, any more than it is a logically sufficient basis for a presumption for or against any specific alteration of policy.<sup>9</sup>

Does it follow that without policy presumptions we make policy decisions without giving thought to the fundamental implications of the policy steps taken? Only superficially does this appear to be the case. To be sure, any analyst, regardless of method, may miss the funda-

<sup>7</sup> Milton Friedman, "A Monetary and Fiscal Framework for Economic Stability," *Am. Econ. Rev.*, June 1948, XXXVIII, 246.

<sup>8</sup> Although use of such a policy presumption is possible only if values are postulated in the conventional manner discussed above, it misses the point to say that its use is merely a special case of the use of postulated values. The point turns not on the separation of value analysis from empirical analysis, which is at the heart of the third distinction between conventional and incremental analysis, but instead turns on the derivation of evaluations of particular policy alternatives from prior evaluations of large institutional complexes.

<sup>9</sup> Behind the conventional presumption there sometimes lies the implicit assumption that market controls and government control are in competition with each other. In incremental analysis lies the contrary assumption that they supplement each other, are indeed essential to each other, and compete with each other only at certain margins.

mental implications of a policy. But when we refuse to employ a *presumption* for competition, we are not therefore logically bound to be indifferent to the implications of a policy for the competitive process.

#### V. *Some By-Products of Clarification*

Presumably we make our common methods explicit in order to inspect and improve them, as well as to communicate them. In the case of incrementalism, improvement in communication is no small gain, especially in view of the difficulty some foreign scholars meet in understanding how we can so easily dispense with the ideological issues around which their own policy analyses revolve. But in addition, clarification produces by-products.

From an understanding of the nature of incremental analysis, some clarification of "muddling through" may be obtained. It appears superficially that "muddling through" sometimes works brilliantly and at other times produces almost farcical errors in policy. The difference in results is understandable on the hypothesis that "muddling through" has come to embrace both highly skilled incremental analysis at one extreme and near refusal to think at all at the other. Similarly the British reputation for inarticulate decision-making and the French reputation for extreme articulateness and theoretical refinement of issues may be attributable to the difference between the incrementalist who needs only to articulate significant differences and a kind of nonincrementalist who attacks each policy problem as though a whole economic or political system were called up for analysis.

The nature of incremental analysis also suggests a possible explanation of certain kinds of analytical noncomparability. Not infrequently a student or an acquaintance who is neither a professional scholar nor a public policy-maker will in discussion propose a somewhat original solution to a policy problem. The proposal is usually put in the form, "Why not . . .?" or "What would be the matter with . . .?" Sometimes the proposal is a good one. But where I think it is not, I am only sometimes able to give reasons for looking askance at it. In a number of puzzling cases, I am unable to do so. I believe my difficulty is not always due to difficulties in language or lack of technical competence.

More than to these factors or to some unspecified difference in background, my inability to provide reasons for rejecting a proposal is perhaps due to my employing successive incremental comparisons and to my inability to fit the new proposal into my customary sequence of comparisons. The student or acquaintance may be thinking nonincrementally or—more probably—with a sequence different from mine. Why might our chains of successive comparisons be different? Because mine but not his is a somewhat specialized professional chain shared with a group of scholars who through similar professional experience

come to a relatively high degree of agreement on the sequences of choice they consider relevant.

This hypothesis may answer such a question as why over the past thirty years or so economists have given short shrift to a number of proposals for monetary reform that were not crack-pot. They arose from a series of incremental comparisons different from the one or few that major groups of economists were implicitly working with. Of one scheme for monetary reform, Albert Hart writes revealingly: "... the scheme has probably not had as much attention as it deserves. The author has been rather a lone wolf in the academic profession. His notions do not fit easily into either academic or political discussion."<sup>10</sup> Such a passage can be read not as a comment on regrettable narrowness in the economics profession, which is what it superficially would appear to be, but as a piece of evidence that, in the employment of incremental analysis, analysts of substantially different backgrounds often cannot compare their results, cannot "appreciate" each other's conclusions.

And as for our difficulties in international comparisons of economic policy, each country's policy is also the end product of a distinctive succession of policy steps. Each policy has been chosen not so much by reference to some standard or postulated goal as by historical comparison with alternative increments. Since each country "begins" its development from a unique position and since each country develops a unique succession of incremental adjustments in both values and policies, it follows that different countries' policies often cannot be compared.

But just as explication of incremental analysis indicates why policies are often noncomparable, it points the way to making them comparable under circumstances in which we have often failed to find comparability. Once it is understood that policies chosen are the product of unique incremental sequences, it becomes clear that to some degree they can be compared if one is willing to take the trouble to work backward through each sequence of comparisons through which the respective policies were produced until some comparable elements are uncovered.

Finally, having found incremental analysis to lean on both incremental politics and incremental attitudes among the citizenry, it remains to be added that incrementalism in the large becomes potentially a major political orientation, or even a philosophy pertinent to still other areas of choice. To explore this would carry us too far afield. In politics, however, we find it increasingly difficult to apply the right-left or conservative-liberal dichotomy; this may be symptomatic that a growing number of individuals are neither conservatives nor liberals but philosophic and political incrementalists.

<sup>10</sup> A. G. Hart, *Money, Debt and Economic Activity*, New York 1948, pp. 445f.

## CAPITAL LONGEVITY AND ECONOMIC DEVELOPMENT

By RUDOLPH C. BLITZ\*

Diametrically opposite analytical conclusions and policy recommendations have been reached in recent discussions on the proper choice of capital longevity. These discussions deal mostly with the issue of capital longevity within the framework of problems of underdeveloped countries. It is not necessary, however, to deal with the longevity problem in such a restricted manner; virtually everything that need be said about capital longevity actually holds for any stage of development.

Thus W. A. Lewis argues that it is a frequent fault of development programs to build capital structures for an unduly long life and too solidly. These are luxuries which underdeveloped countries can ill afford because capital is so scarce for them [8, p. 395]. On the other hand, Galenson and Leibenstein hold that long-lived capital offers to underdeveloped areas the great advantage of a longer period during which no replacement has to be made. This in turn, makes possible a greater net output and greater reinvestment per man, which may be crucial for overcoming a critical early stage in the development process [4, p. 362].<sup>1</sup>

Neisser, commenting on the Galenson-Leibenstein article and considering the issues of lesser cost of short-lived equipment versus the gain of postponed replacement of more durable equipment, states: "I do not know of a criterion for weighing the advantages of a more rapid initial growth of operating capital against the disadvantages of an earlier temporary interruption" [10, p. 646].

\*The author is assistant professor in the department of economics, Northwestern University. He is greatly indebted to Fred Westfield for highly lucid suggestions and criticisms and for the appended mathematical note. Robert Eisner, Karl de Schweinitz, Robert Strotz and Stefan Valavanis have also provided stimulating ideas.

<sup>1</sup>Galenson and Leibenstein cite Domar's calculations which show, for example, that with a rate of growth of gross investment of 5 per cent per year and an average length of life of capital of 4 years, the equilibrium ratio of replacement to gross investment will be 82 per cent per year. If the average length of life of capital should be 10 years, the ratio of replacement to gross investment drops to 61 per cent; and if the average length of life of capital is 30 years the ratio of replacement to gross investment would be merely 22 per cent per year [2, p. 8]. Domar, however, makes it quite clear that more long-lived equipment, while it has the agreeable quality of postponed replacement, also has the disagreeable quality of being more expensive; he states explicitly: "There is no doubt that beyond a certain range greater capital longevity becomes very expensive . . ." [2, p. 7]. Galenson and Leibenstein overlook this reservation of Domar.

In their "Reply" Galenson and Leibenstein admit that "... long-lived investment implies less output per unit of capital and therefore less reinvestment per unit of capital" [5, p. 647]. They nevertheless maintain their original position in favor of long-lived capital.

Thus the issue stands, and neither the advocates of short-lived or long-lived capital have stated with any precision just exactly how short-lived or long-lived they think capital should be, or what criteria they would use to make a decision in specific cases. Common sense would suggest that the advocates of neither position would propose the most extreme alternatives. Lewis would hardly advocate capital equipment with zero or close to zero life for all underdeveloped areas and neither would Galenson and Leibenstein advocate under all circumstances and at any cost the most long-lived capital equipment obtainable—even if one abstracts from the problem of obsolescence. In the subsequent discussion we shall develop a model which, we believe, contains the relevant criteria for the determination of optimum longevity of capital equipment for any stage of economic development.

### *I. Long-Lived versus Short-Lived Capital*

All capital goods can be conceived as having stored up in them a certain volume of physical output or service which they release in form of a flow during their lifetime. In order to avoid ambiguity we shall assume that a machine which has a lifetime of  $k$  years does not deteriorate for  $k$  years but falls apart and has no scrap value at the end of the  $k$ th year. We can then distinguish two types of capital goods with respect to their longevity: (1) Physical service (or output) is rendered in annual fixed installments. It is, however, possible for capital goods yielding the same annual service to have different longevities. For example, roofs made of different materials yield the same annual square footage of shelter, but for different lengths of time. (2) The annual rate at which physical service (or output) is released can be varied; within limits this type of capital goods can be used more or less intensively without affecting the total output which the capital good yields over its lifetime. The respective chronological life-spans of two machines of this type will be determined by differences in the stock of service and by differences in the intensity of use.<sup>2</sup>

#### *A. The Cost of Longevity: Present Supply Price*

In the case of two pieces of capital equipment producing the same commodity or service at the same rate per unit of time, the piece with

<sup>2</sup> E.g., machines can be run for one to three shifts without penalty to their lifetime output, but their chronological life-span will depend on the number of shifts the machines are run. The chronological life of a machine "containing" 300,000 units of service will be no longer than that of a machine "containing" 100,000 units of output if the first one is operated on a three-shift schedule and the second one on a one-shift schedule.

the longer life-span, which thus achieves the greater output during its life, will cost more than the machine with the shorter life-span. The same holds true for a machine capable of yielding a larger number of units during its total life compared to one which yields fewer units, but where the rate of output and thus the chronological life, can be varied.

We may suppose that there exists a wide range of machines, each producing at the same rate of annual output, but producing it for different lengths of life.<sup>3</sup> Then we may think of a total cost schedule  $A = A(k)$  which relates the initial cost of the machines,  $A$ , to their longevity,  $k$ . This is a technical or engineering relationship. From this total cost schedule one may derive an average cost curve, which we may call  $a = a(k)$ . It is the total cost of the machines divided by length of life; it shows the amount of annual "straight-line" depreciation of the machines as a function of longevity. As usual, where there is a total or average cost schedule, there is also a marginal cost schedule; we shall call it  $A' = A'(k)$ . If  $A(k)$  is continuous—although, of course, it may have corners—then the cost of increments of longevity are defined for every value of  $k$ .

In very many instances the average and marginal cost curves of longevity are likely to be U-shaped. Of course, there may be very great variety in the shape of the individual "U's." The production of any piece of durable equipment, be it a man's suit or a locomotive, involves certain construction or assembly costs which over a wide range will be frequently independent of the durable quality of the material used in the construction of this piece of equipment. The case here is somewhat similar to one factor which makes for economies of scale, namely, that up to a certain point the assembly cost of a smaller or a larger piece of machinery may be the same and this, therefore, will result in a scale advantage in the case of the larger piece of equipment. On the other, in the case of longevity, after a certain point is reached additional years of life will be subject to increasing cost increments both because the price of the more durable material may increase very drastically and also because the exacting workmanship, care in assembling the parts and inspection of the finished product, which all make for greater longevity, become very expensive.

For convenience we will assume that  $A(k)$  starts at the origin and therefore for  $k = 0$   $a = A'$ . This means that there exists a continuous choice for longevity. The  $a, A'$  curves are drawn on these assumptions in Figure 1. We may call these cost curves "objective" cost curves; they indicate the terms on which buyers can purchase longevity.

For our purposes it is unnecessary to linger on the question whether

<sup>3</sup> The lifetime of a machine was defined previously as the period of  $k$  years during which the machine yields service without deteriorating and this lifetime is terminated by sudden, complete disintegration of the machine.

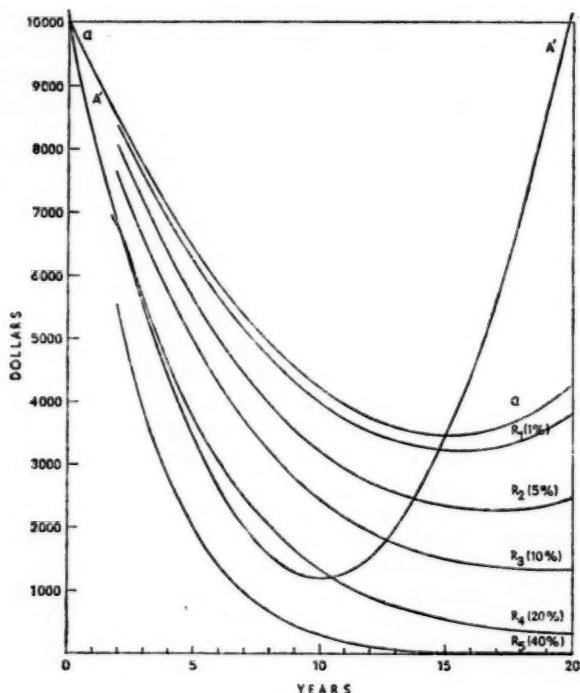


FIGURE 1

a U-shaped marginal longevity cost function constitutes the general or merely a special case. This is a matter which should be settled not on a priori grounds but by engineering research. All that is needed for our argument is that additional longevity costs something. We will consider both U-shaped marginal cost functions and a constant marginal cost function.

### B. Optimal Longevity<sup>1</sup>

We may divorce the problem of optimum longevity from demand and variable-input considerations by considering in isolation the problem of determining what constitutes—as far as longevity alone is concerned—the lowest cost method of production. If machines of alterna-

<sup>1</sup>This topic is treated more precisely in the appended mathematical note. A similar solution for optimal longevity has been reached by Frederick and Vera Lutz [9, pp. 120-22]; also see R. G. D. Allen [1, pp. 404-5]. After completing this manuscript my attention was called to the discussion of Gustaf Akerman's *Realkapital und Kapitalzins* by Knut



tive longevity respectively (1) produce at the same rate of total output per year and (2) require the same rate of variable input per year, then the annual "gross profit" per year (annual gross revenue minus annual variable cost) will be the same for all the machines regardless of longevity. Moreover, in order to put machines of varying longevity on the same footing, we shall suppose that a current investment commitment for a machine of  $k$  years longevity obliges us also to replace the machine at the cost of  $A(k)$  every  $k$ th year for all eternity.

If we make these simplifying assumptions, we find that current investment commitments of  $A(k)$  and its perpetual replacement have a present equivalent cost which will depend solely upon longevity and on the rate of interest,  $r$ .<sup>8</sup> The latter is equal to the marginal efficiency of capital in the economy and is assumed to be unchanging over time. This present equivalent cost is designated as  $C = C(k, r)$ . Then, if we subtract from  $C(k, r)$  the initial cost of a machine of longevity  $k$ , i.e.,  $C(k, r) - A(k)$ , we obtain the present discounted cost of all future replacements.

When  $r$  is given and constant over time, the optimum longevity is obtained where  $C$  is a minimum.<sup>9</sup> Since neither annual revenues nor variable cost depend on  $k$ , if any investment at all is to be made, it must be of this longevity. As longevity is increased by an increment this brings about an increase in total objective cost,  $A$ , but it also brings about "savings." The savings arise from the fact that as  $k$  is lengthened the resources for all future replacements will be needed an increment later in time every time the replacement has to be made; and this holds forever. These savings are a sort of capital gain. The optimal condition will therefore be achieved when the initial increment in outlay,  $A'$ , equals the capitalized value of all future savings arising from these

Wicksell [13, pp. 258-99]. I was pleased to discover that there was already here the foundation of my analysis.

George Terborgh develops the concept of "operating inferiority" [12, pp. 61ff., 74-91, 168ff]. This concept refers *inter alia* to increases in operating costs due both to old age of the machine and to its obsolescence. Thus the accumulating "operating inferiority" is calculated by comparing the operating costs of the machine in actual operation to those of a new machine of the same type and also by making allowance for obsolescence on the basis of a comparison of operating costs of the machine in operation and those of later improved models. The initial cost of a machine together with its accumulating "operating inferiority" and the prevailing interest rate determine the policy for replacement. By contrast our model assumes that operating costs are unaffected by the age of the machine and it abstracts from obsolescence. Our model also assumes that differences in the initial cost of the machine are solely due to differences in longevity.

<sup>8</sup> In the absence of risk the "natural" rate of interest would seem to be the proper one for any economy to utilize for the allocation of scarce resources.

<sup>9</sup> The appended mathematical note shows that under the assumptions of a U-shaped average and marginal longevity cost curve,  $C$  as a function of  $k$  will have a regular minimum at least for some interest rates.

postponed replacements. The savings can be visualized as a "marginal revenue" of increased longevity. It is these savings which Galenson and Leibenstein rightly emphasize. However, they neglect the cost side of the problem. Only as long as the marginal cost of longevity is less than the marginal revenue of longevity is it economical to increase the total cost,  $A$ , of longevity. Five hypothetical marginal revenue curves, which of course depend on the nature of the total cost functions themselves, are labeled  $R_1$  to  $R_5$  in Figure 1 for interest rates ranging from 1 to 40 per cent.

The optimal conditions are discussed more precisely in the appended mathematical note. In general, the higher the interest rate, the smaller the optimal value of  $A'$  and  $k$  will be, because the present value of the future savings, discounted at a higher interest rate, will be less. As the interest rate approaches zero, the appropriate longevity approaches the minimum of the objective longevity average cost curve, but will always be somewhat to the left of it.<sup>7</sup> If the average and the marginal cost of longevity are constant, the proper choice of  $k$  is the very lowest value possible, no matter how high or low the interest rate, just as long as it is positive. This case can easily be understood; the reader need merely ask himself which machine he would pick, if given the choice between machines  $M_1$  lasting two years and costing \$2000 and machine  $M_2$  lasting four years and costing \$4000. The proper choice is obviously  $M_1$ , given a positive rate of interest, as interest can be earned on the \$2000 which becomes freed if the lower-priced machine is purchased.

Hence some longevity will be purchased as long as the objective average cost of longevity is falling, the exact amount depending on the shape of  $A'$  and the interest rate; but only the least possible amount of longevity will be purchased if the  $a$  and  $A'$  cost functions are virtually a horizontal line. This conclusion is really very much in accord with Böhm-Bawerk's notions on the roundaboutness of capital and its virtues and vices. When the average cost of longevity is falling, additional longevity (roundaboutness) has additional virtues up to a certain point, and it is therefore worth while to purchase longevity; whereas if the average cost of longevity is constant, additional longevity would constitute an unnecessary burden.

### C. *Expectation of Future Fall in the Interest Rate*

It is quite possible that an underdeveloped country which is initially plagued by a scarcity of capital and by high interest rates may nevertheless anticipate a substantial fall in the rate of interest. This may

<sup>7</sup> If the interest rate is so high that no intersection between the  $A'$  and the  $R$  function takes place, in our case at 40 per cent, this would mean that the appropriate policy would be to utilize machinery of zero longevity, that is, to operate without the piece of capital equipment under consideration and to rely instead on hand methods.

be so because more savings are anticipated from a growing national income, or because it is hoped that the country will become more "credit-worthy" for both foreign and domestic lenders after having made a good start on the road to economic development, or because of both these factors.

In this situation the optimal choice of longevity will be affected. It would be perfectly rational to buy even less longevity at a high interest rate, if it is expected to fall, than would be purchased if the high interest rate were expected to prevail. If a fall in the interest rate is anticipated, the appropriate choice is to store up only very little longevity for the time being and to buy more of it later on, when the interest rate has fallen. A fall in the interest rate is really the same thing as a fall in the price of longevity itself. This situation is similar to that of a man who, when down and out, might borrow \$25 from a usurer to purchase a cheap suit in order to make himself appear respectable and thus obtain a job. This person may know full well that the suit will wear out in six months and that a \$50 suit would last two years. This choice, however, was not open to him at that time, or rather the interest on the additional \$25 necessary would have been prohibitive. However, after having found employment, he will eventually replace his \$25 suit with the \$50 suit.<sup>8</sup>

#### *D. The Cost of Longevity and the Gestation Period*

Starting from the point of time of the investment decision it will very frequently take longer to put long-lived capital equipment into actual operation than short-lived equipment. This problem is merely a variation of the more general proposition that long-lived capital equipment is more expensive; it nevertheless warrants separate discussion since it may be of special importance for underdeveloped areas.

Frequently the difference in longevity between the more and the less expensive piece of capital equipment is due to the use of more labor, tools, and material in the construction of the more expensive, longer-lived piece. In this instance the period of construction for the long-lived equipment can only be the same as that of short-lived equipment designed for the same task if it is possible to use labor and/or tools more intensively in the first case than in the second case. The intensification and shortening of the construction process may become very expensive, because it may require premium payments such as overtime. Furthermore, especially in the underdeveloped areas, where there is a shortage of skilled labor and of equipment to build equipment, it may be quite impossible to speed up the construction of the more long-lived equipment. There is, for example, a limit on how many men

<sup>8</sup>We assume the suits to be similar in all other respects except longevity.

can work on the face of a rock or even on two faces in the construction of a tunnel. It may therefore be a perfectly rational decision to construct "in the mean time" a cheap railroad bed which travels around the hill, although such a route may appear "uneconomical" in the long run *if time preference is ignored* as compared to the more direct and more durable roadbed which would instead go through the hill.

Many mining shanty towns or city slums of the United States are the almost necessary by-product of a very rapid pace of industrialization.<sup>9</sup> In military operations, where time preference is extremely high, it is standard procedure to build a temporary bridge and to replace it subsequently with a more permanent structure. Similarly, and the analogy is an uncomfortably close one, developing countries must also find means to cross their rivers in a hurry (*cf.* [4, p. 369]).

The more long-lived equipment, which in any case requires more resources, becomes even more expensive as compared to the less long-lived equipment because of the additional cost of waiting. The magnitude of this additional cost will depend on the rate of interest and the size of the expected income stream. The larger the anticipated income stream the greater is the cost of waiting. Many basic investments in underdeveloped areas, such as roads, housing, schools and clinics may provide external economies for many other ventures. To the extent that any industry provides external economies to other industries, the delay and cost arising from the penchant for longevity will be compounded once more. In terms of Figure 1 the effects of a gestation period can be visualized as either an upward shifting of the  $A'$  curve or downward shifting of the appropriate  $R$  curve.

## II. Maintenance Costs and Obsolescence

In the formal model used so far optimum longevity was determined solely by the marginal longevity cost function,  $A'$ , and the interest rate,  $r$ . We assumed perpetual replacement, which implies perfect foresight and the absence of all technological change. We also assumed a constant rate of output over time in perpetuity, and constancy of both variable factor input and product prices. We abstracted from such issues as maintenance costs and obsolescence. In the real world the relationship between longevity and the rate of interest may turn out to be much different from that suggested by our simplified model; this divergence may be due largely to the role of maintenance cost and obsolescence.

The term maintenance cost will be used here as a concept quite distinct from fixed and variable costs. Whether or not a certain out-

<sup>9</sup> Housing facilities in the newly industrialized areas of the Soviet Union too are reported to be very crude and frequently of a makeshift character (*e.g.*, [6, pp. 186-87]).

lay constitutes a maintenance cost according to our definition depends on two criteria, both of which must be satisfied simultaneously: (1) an outlay on service of equipment must extend the longevity of the equipment; (2) the outlay can be undertaken only after the equipment has been installed and has been in operation for some time.

To make the concept of maintenance costs quite clear it is necessary to introduce another concept which we will call durability outlays, which are quite distinct from maintenance costs. Durability will refer to an enduring quality of equipment which is achieved without maintenance outlays. The concept of durability is also distinct from our earlier concept of longevity, which is broader. Longevity, we will now recognize, can be bought as some mix of durability and/or maintenance outlays, subject only to the restraint that it must contain *some* durability, but it need not contain any maintenance outlays.<sup>10</sup> In Section I we abstracted from maintenance costs and therefore the broader concept of longevity became synonymous with the narrower concept of durability as those concepts have here been defined.

The following examples will make clear the distinction between our concept of maintenance costs on the one hand, and fixed, variable, and durability costs on the other hand. The periodic blasting of loose rock from the face of a mountain, which is traversed further down by a highway or a railroad, is frequently referred to in common language as maintenance of way. In economic terms, however, this constitutes simply a fixed cost which has to be undertaken periodically to allow the safe passage of traffic; it is a fixed cost because it is independent of the volume of traffic. On the other hand much of what is commonly called maintenance cost constitutes really variable cost in the economic sense. The removal of ashes and slags from a furnace is simply a variable cost and is directly proportional to the rate of input of coal and other raw materials and is, of course, also related to the rate of output of this furnace. Other cleaning operations, commonly referred to as maintenance costs—such as the removal of accumulated lint from cutting machines—are of the same nature.

In what follows we will deal primarily with the appropriate principle of choosing between durability and maintenance outlays. A very simple example of such a choice would be whether to use for construction purposes material which costs more initially but need not be painted, or to use less expensive material which must be painted periodically in order to last as long as the unpainted material.

If certain expenditures on maintenance are necessary to make a

<sup>10</sup> A somewhat loose analogy may help here: the life-span of a human being may be due to two factors. One is his innate physical endowment (durability), the other is the intensity of effort of the doctors (maintenance).

machine last  $j$  years longer, then these these maintenance costs over the lifetime of the machine can be discounted to the present and can be treated in our framework as part of the initial total outlay  $A$ . It should be clearly understood that while we treat discounted maintenance costs as a substitute for greater outlays on durability for the purpose of extending longevity, this does not imply that if longevity in the form of durability of  $m$  years is extended to, say,  $m + 5$  years, this can be done simply by spending zero on maintenance for  $m$  years and then incurring certain annual outlays on maintenance for the next five years. It

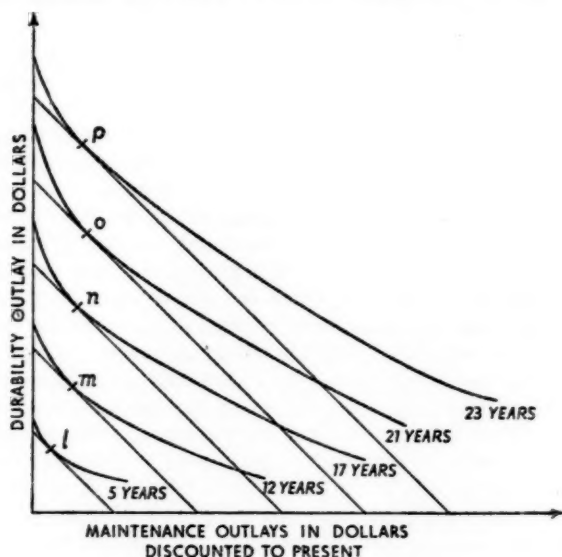


FIGURE 2

may be necessary to spend part of the maintenance outlay long before the expiration of  $m$  years.

The relationship between durability and maintenance is shown in its simplest form in Figure 2 with isoquant curves. A more precise geometric exposition will be given subsequently. These isoquants can be drawn either on the assumption of a zero interest rate or on the assumption that all "maintenance-work hours" are discounted appropriately to the present; in the latter case, of course, the shape of the isoquants themselves would change with changes in the interest rate. Moreover, each present discounted value of future maintenance expenditures located on any isoquant represents an optimal pattern of maintenance-work hours through the time which can be purchased with a certain

amount of cash on hand today. The family of 45 degree lines are constant outlay curves which show that a dollar spent on durability can be substituted for maintenance outlays whose present discounted value is a dollar. The isoquants start from the Y-axis because, as mentioned previously, longevity can be obtained by some outlay on durability without any additional maintenance outlay, but not with an outlay on maintenance and no outlay on durability. From the points  $l$  to  $p$  both total and marginal longevity cost curves can be constructed. These points minimize the total present costs for specified amounts of longevity.

The same relationship can also be shown by means of a conventional total cost curve. Curve  $A$  in Figure 3 is a total durability cost curve.

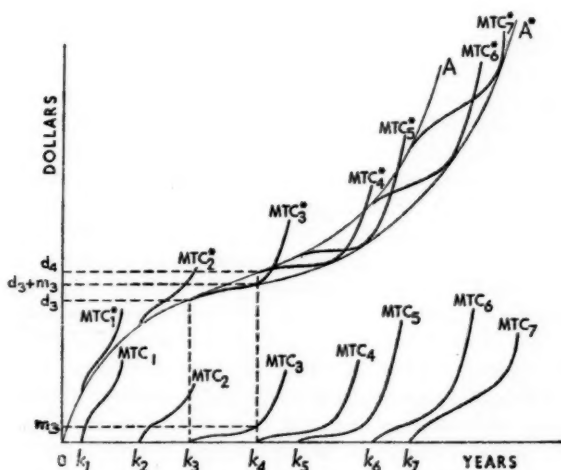


FIGURE 3

It indicates the dollar cost of varying durabilities or, alternatively, of varying longevity if no maintenance costs are to be incurred. If, for example, a durability of  $k_4$  years is to be acquired the cost will be  $d_4$  dollars. If a machine of that durability has been purchased, however, its lifetime (longevity) may be extended beyond  $k_4$  years upon incurring maintenance costs. The maintenance costs necessary to extend the longevity of this machine beyond  $k_4$  years vary in accord with the maintenance total cost curve  $MTC_4$ . To attain various longevitys in excess of  $k_4$  years by using a machine of  $k_4$  years durability and maintaining it so that it lasts longer, the combined durability and maintenance costs will vary according to  $MTC_4^*$ . Such a curve is obtained by adding



to the *MTC* curve the cost of the durability which defines the *MTC* curve. Thus  $MTC_4^* = MTC_4 + d_4$ .

Consider next the envelope of the *MTC*\* curves. This envelope curve is *A*\*. It shows the minimum cost of obtaining any prescribed longevity by an optimal combination of durability and maintenance outlay. To obtain longevity  $k_4$ , for example, the optimal durability cost would be only  $d_3$  which provides a durability of  $k_3$  years. Then maintenance outlays of  $m_3$  dollars would be incurred to extend the life of this machine to  $k_4$  years, as shown by the curve *MTC*<sub>3</sub>. The total cost of that longevity would then be  $d_3 + m_3$ , the height of the curve *A*\*.

We can now transform the total cost curves of Figure 3 into average cost curves (see Figure 4). Corresponding to our earlier graph (Figure 1) and to the *a* and *A'* curves we can now construct two new curves which we label *a*\* and *A'*\*, the latter two being derived from the *A*\* curve (Figure 3). The *a*\* curve is an envelope curve and will be the same as curve *a* for any range in which additional longevity is obtained more cheaply by buying more durability than by incurring more maintenance expense; but it will be below the *a* curve and will be an envelope of the average joint longevity cost curves (labeled *AJLC*) in any range in which some additional longevity is purchased more cheaply in the form of maintenance rather than in the form of greater durability. *A'*\* shows the marginal cost of longevity, given an optimal allocation of expenditure on durability and maintenance, and supplants the role of the curve *A'* in Figure 1 in determining the optimal longevity.

The envelope curve *a*\* is in many ways similar to the more familiar envelope curve used for the exposition of economies of scale. The similarity consists in the fact that we get, as long as the envelope curve is falling, a phenomenon of undermaintenance at the optima, just as one gets underutilization in the case where the envelope curve of conventional cost analysis is falling because of economies of scale. It is always worth while, if additional longevity is desired, to buy more durability and to undermaintain it (*i.e.*, not to maintain it to the point where the *AJLC* is minimal) as long as the average cost of durability is falling, rather than to buy less durability and to operate where average maintenance cost is at a minimum.<sup>11</sup>

<sup>11</sup> Our use of the concepts of durability and maintenance may appear artificial, especially the notion of obtaining durability without maintenance outlays. Therefore, before pointing out specific practical implications of this model, it should be explained that the model presented here has been designed with a particular sort of capital in mind. We have thought in terms of capital such as roads, bridges, and buildings which have significant durability even in the absence of maintenance outlays, although maintenance outlays may extend their longevity further. Our model may be less adapted to instances of capital equipment which can have only very short durability without substantial maintenance expenditure. In such a case the envelope curve *a*\* may be formed, over most of its range, by a single *AJLC* curve.

If maintenance costs are incorporated in the  $a^*$  and  $A'^*$  functions and if maintenance costs differ as between countries because of the different relative scarcities of the requisite labor, we will actually obtain distinct  $a^*$  and  $A'^*$  curves for each individual country even though durability costs are the same. This is a substantial modification of our

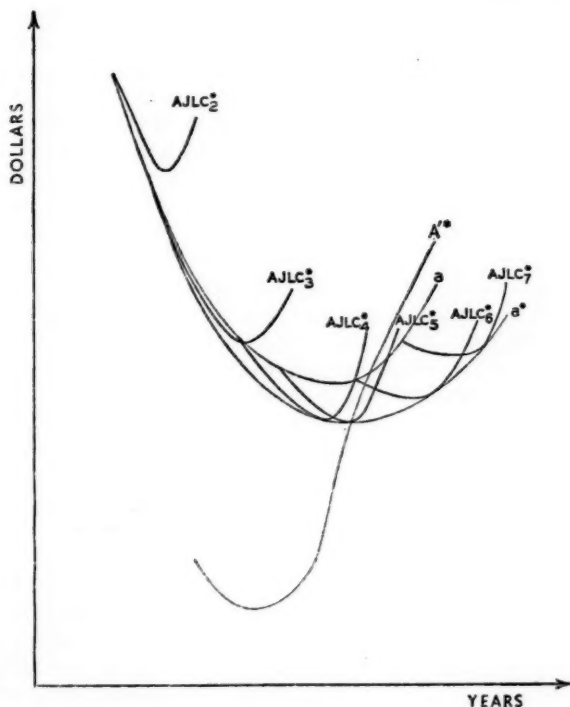


FIGURE 4

earlier model where we used common  $a$  and  $A'$  curves as between different countries, and different longevity decisions were solely due to differences in the interest rate.

A relatively ample supply of labor should help to keep maintenance costs low, although what really counts is not abundance of aggregate labor but of appropriately skilled labor. Thus maintenance costs may be extremely high in underdeveloped countries with an abundance of cheap but unskilled labor. But maintenance costs may be low in, say, industrialized Europe with aggregate labor much scarcer but skilled

craftsmen making up a much higher proportion of the total labor supply.

Moreover, at the time the capital equipment is installed, maintenance costs in contrast to durability outlays appear as postponable expenditures. Some maintenance expenditures, to be sure, cannot be postponed for very long, but others—and these are likely to be the more substantial expenditures—only loom in the more distant future. Countries where efficient labor is relatively abundant, and which for this reason have relatively low maintenance costs, are also likely to have to contend with higher interest rates. For both reasons maintenance outlays are likely to be substituted for durability outlays in these areas. These considerations may help to explain why countries which suffer from much greater capital scarcity than the United States may yet turn out to use capital equipment of greater longevity than is used in the United States.

Not only will the amount of maintenance expenditure per period on a certain type of machine differ from country to country depending on the cost of maintenance, but there are differences in the maintenance techniques employed which are attributable to differences in the relative factor scarcities in different countries. These differences in maintenance techniques may have many subtle effects on the design of capital equipment. For example, in contrast to British open hearth furnaces, American furnaces are designed to allow free access to the vulnerable parts of the furnace for mechanical handling equipment, such as high-lift and fork-lift trucks. On the other hand, English repair methods rely less on mechanical aids, but are more labor intensive and require more time.<sup>12</sup>

We have glossed over the difficulties of international empirical comparisons of longevity. Because maintenance costs would be different, we would expect to get different longevity cost curves for different countries. But because of different relative scarcities of factors in different countries it would be virtually impossible to find capital equipment which is the same in all other aspects and differs only in regard to longevity. An interesting illustration of this problem is the blast furnaces used in the United States and Great Britain. On the one hand American furnaces are lined with more expensive material than their British counterparts, and should therefore be more durable. This greater durability, however, is offset by higher driving rates and greater wind velocity applied in the American smelting process. Comparisons are further

<sup>12</sup> In this instance the difference in maintenance technique accounts not only for a different design of the capital equipment, but because the American maintenance techniques are less time-consuming than the British, the average ratio of idle furnaces to total furnace capacity is much lower in the United States than in Britain. [14, p. 99]. The adaptation of equipment to peculiar "maintenance scarcities" is also a paramount problem in the design of much military equipment.

beclouded by the fact that American furnaces are on the average larger and leave both a different volume and quality of slag from those used in Britain. Moreover, the coke used in the United States has a lower sulphur content than British coke. The net result of all this is that the average life of American furnace linings is slightly less than that of British linings but a greater tonnage is achieved by American furnaces between relinings [14, pp. 33-34].

There is a widespread belief that both the average age of much European capital equipment is greater and also its life span is longer than in the case of its American counterparts.<sup>13</sup> This view has been presented by L. Rostas [11, pp. 55-58]. He also indicates that there is very little reliable information on this matter and is therefore forced to leave the whole question unsettled. The reader of this paper is cautioned that much capital goods used in different countries for similar purposes will turn out at best to be different species of the same genus.

Expected obsolescence and expected changes in the demand for the product will, of course, also have to be taken into account in making decisions as to the longevity of equipment that is desirable. Although a rigorous analysis of the necessary modification in our model to take these variables into account is not possible, a few general observations are in place here. If a precise comparison could be made between the United States and industrial Europe as to the effects of expected obsolescence on capital longevity, it might be found that expected obsolescence has created more of a bias in favor of short-lived capital equipment in the United States. Because for long periods the rate of progress expected in the United States has been more rapid, or merely because of fashion-induced style changes, much capital equipment and also durable consumers' goods may have been constructed for a shorter life span than has been the case in Europe.<sup>14</sup>

On the other hand, anticipated obsolescence and similar uncertainties may be especially important considerations for countries which are just passing through the initial stages of industrialization and which expect very drastic—not to say revolutionary—technological and social

<sup>13</sup> Comparisons as to the average age of capital equipment of course reflect longevity of equipment, but they also reflect the historical path of the preceding birth and death rates of the machine population. An investigation for Western Germany shows that in 1952, 30 per cent of German machine tools were less than 10 years old and 69 per cent less than 20 years old [15, p. 44]. In the United States as of the same date 54 per cent of similar equipment was less than 10 years old and 79 per cent less than 20 years old. It is not unreasonable to assume that this difference may in part be due to the fact that in Germany recovery based on rearmament proceeded very rapidly after 1933, whereas the recovery lagged behind in the United States by several years.

<sup>14</sup> It is interesting to observe that both the Rolls-Royce and the German Volkswagen, automobiles on opposite ends of the price range, strongly emphasize the absence of fashion changes in their sales appeal.

changes.<sup>15</sup> Anticipated changes of this sort then will push the choice even further in favor of short-lived equipment, made already on the basis of the capital scarcity prevailing in these countries.

### III. Conclusions

A model has been presented in which the optimum life of capital equipment was related to the interest rate, the longevity cost function, and the substitution of maintenance outlays for durability. In the first section of the paper maintenance aspects of the problem were ignored with the result that optimal longevity was inversely related to the level of the interest rate. This solution was the result of the fact that the discounted value of "savings" of postponed replacement would become less the higher the interest rates. This might be referred to as the direct effect of the interest rate on longevity. Nonobservance of the rules established here will result in misallocation of resources in the same manner as nonobservance of other rules of resource allocation.

Then the maintenance aspects were explored explicitly. It was shown that the interest rate has a pronounced effect on the substitution between the amount of initial capital expenditure and maintenance outlays. In the language of this paper this involves a substitution between durability and maintenance outlays and this, in turn, results in movements of the longevity cost curve. This might be referred to as the indirect effect of the interest rate on longevity.

The interest rate, therefore, really affects capital longevity in two different ways: One is the simple and direct impact on longevity. The second effect is of an indirect nature. It asserts itself through determining *inter alia* the rate of substitution between durability and maintenance outlays. The higher the interest rate the more heavily should maintenance be substituted for durability. Of course the possibilities and the nature of such substitution are primarily determined by technological considerations. The direct and the indirect effects of the interest rate, just discussed, really have opposite impacts on optimum longevity.

Areas in different stages of economic development should vary systematically in their choices between durability and maintenance, and these choices will have implications for the appropriate longevity of capital. If high interest rates and low maintenance costs prevail in a country, then maintenance outlays should be substituted for durability outlays. In this case the substitution of maintenance outlays for durability would overshadow the direct discouraging effect of high interest

<sup>15</sup> E.g., A. E. Kahn emphasizes the "... additional leaks, lags and frictions ..." which make investment undertakings in primitive economies unusually risky and subject to the danger of rapid obsolescence [7, pp. 50-51]. For a similar view also see W. A. Lewis [8, p. 395].

rates on longevity. Thus it may be the correct policy under these circumstances for countries where high interest rates prevail to decide nevertheless on capital equipment of greater longevity than would be appropriate for countries where the interest rate is lower. Furthermore, if expectations of obsolescence are lower in the former as compared to the latter case, this should reinforce the pattern of choice just discussed. It is a shortcoming of the formal analysis of this paper that it has not come to grips satisfactorily with problems of risk and uncertainty.

Although most maintenance work is likely to be very labor-intensive and although most underdeveloped countries are blessed with an ample supply of labor this does not imply that these countries should invariably spend heavily on maintenance and stint on durability. Some maintenance work requires little skill and in these cases the choice should obviously be in favor of maintenance outlays. The problem is more complex in the case of maintenance work which requires much skill and judgment. The cost of very skilled maintenance work may be extremely high in underdeveloped countries despite an ample supply of labor.

In the short run then it would appear as a bad policy for such a country to attempt to substitute much of this type of maintenance for durability. We cannot within the framework of this paper discuss the issue of investment in capital goods versus investment in human beings. Our foregoing analysis, however, has thrown light on at least one relationship which would have to be considered among others, if this choice is to be made correctly. The training of appropriate skills may frequently allow a drastic extension of the longevity of capital equipment and in turn, as has been shown in the beginning of this paper, increased longevity may have most important effects on the rate of economic growth.

## A MATHEMATICAL NOTE ON OPTIMUM LONGEVITY

By FRED M. WESTFIELD\*

1. Under the assumptions made in *Section 1* the optimal longevity is determined by finding the minimum of the expression:

$$\begin{aligned} (1) \quad C(k, r) &\equiv A(k) + A(k)e^{-kr} + A(k)e^{-2kr} + A(k)e^{-3kr} + \dots \\ &\equiv \sum_{j=0}^{\infty} A(k)e^{-jkr}, \end{aligned}$$

where  $r$  represents the given and constant instantaneous rate of interest

\* The author is assistant professor in the department of economics, Northwestern University.

and  $C$ , the present equivalent cost of an investment commitment costing  $A(k)$  initially and again  $A(k)$  every  $k$ th year thereafter.<sup>16</sup> Since this is a geometric progression, for any positive interest rate its sum may be written as (cf. [9, p. 120]):

$$(2) \quad C(k, r) \equiv \frac{A(k)}{1 - e^{-kr}}.$$

2. A necessary condition for the minimum of  $C(k, r)$  is that its first partial derivative with respect to  $k$ ,  $\partial C / \partial k$ , be zero:

$$\frac{\partial C}{\partial k} = 0 = \frac{(1 - e^{-kr})A'(k) - A(k)re^{-kr}}{(1 - e^{-kr})^2},$$

where  $A'(k)$  is the first derivative of  $A(k)$ . This can be simplified to:

$$(3) \quad A' = \frac{A re^{-kr}}{1 - e^{-kr}} \equiv C r e^{-kr} \quad (k, r > 0).$$

This summarizes the conclusion that optimum longevity is determined by the value of  $k$  for which the "objective" marginal (undiscounted) cost of longevity ( $A'$ ) is just equal to the present discounted value ( $C r e^{-kr}$ ) of the savings from the incremental deferral of all replacements—the "marginal revenue."

3. The necessary second-order condition for a minimum is that the second partial derivative of  $C$  with respect to  $k$ ,  $\partial^2 C / \partial k^2$ , must be positive. Carrying out this differentiation and simplifying, we can obtain the condition:

$$(4) \quad A''(k) > -rA'(k),$$

where  $A''$  is the slope of the marginal cost curve  $A'$ , and  $-rA'$  turns out to be the slope of the marginal revenue curve. This inequality is the usual requirement that the marginal cost curve intersect the marginal revenue curve from below. Since  $A'$  is everywhere positive and U-shaped, if the two curves intersect at all this condition will be met at one of the intersections. But even if  $A'$  is not U-shaped, but steadily falling, the condition can still be fulfilled provided that the  $A'$  curve does not fall more rapidly than the marginal revenue curve.<sup>17</sup>

4. A higher interest rate must necessarily reduce the optimum length of

<sup>16</sup> The instantaneous interest rate  $r$  is related to the interest rate  $i$  compounded once per year, by the relationship:

$$e^r = (1 + i),$$

where  $e$  is the base of the natural logarithms. (Cf. [3, pp. 359-62].)

<sup>17</sup> In the extreme case where the marginal longevity cost curve is a horizontal straight line, and the average longevity cost curve  $A(k)/k = a(k)$  is also the same straight line, the first-order conditions hold in the limit as  $k$  approaches zero. Under these conditions  $C(k, r)$  approaches its minimum as a limit as  $k$  approaches zero.



longevity,  $k$ . We differentiate the equilibrium conditions (3) totally with respect to the parameter  $r$ ; or

$$\frac{d}{dr} [A'(k) - C(k, r)re^{-kr}] = 0.$$

After performing the indicated operation and simplifying, we obtain the expression for the desired rate of change of the optimum  $k$  as a result of an incremental change in  $r$ :

$$\frac{dk}{dr} = \frac{-Ce^{-kr}[rke^{-kr}/(1 - e^{-kr}) + rk - 1]}{A'' + rA'}.$$

Is this expression positive or negative? We know that the present equivalent costs,  $C$ , and the discount factor,  $e^{-kr}$ , are positive. We also know that the denominator,  $A'' + rA'$ , is positive; the second-order condition (4) requires this. Therefore, the sign of  $dk/dr$  is governed by the sign of the term in brackets. If

$$\frac{rke^{-kr}}{1 - e^{-kr}} + rk - 1 > 0,$$

$dk/dr$  will be negative; and, vice versa, if this inequality is reversed. Dividing the numerator and denominator of the first term by  $e^{-rk}$  and multiplying the entire expression by  $-1$  times the resulting positive denominator of the first term, the condition becomes

$$e^{+rk}(1 - rk) < 1.$$

This inequality is clearly satisfied for all  $r$  and  $k$  such that  $rk \geq 1$ . Therefore, in these cases,  $dk/dr$  is negative. Is it also satisfied for small values of  $k$  or  $r$  such that  $0 < rk < 1$ ? Taking (natural) logarithms of both sides, one obtains

$$rk + \ln(1 - rk) < 0.$$

This is of help because the second term may be expanded so that we obtain

$$rk + \left( -rk - \frac{(rk)^2}{2} - \frac{(rk)^3}{3} - \dots \right) < 0, \quad 0 < rk < 1,$$

which is obviously satisfied for such small positive values of  $r$  or  $k$ . Hence an increase in the interest rate cannot under our assumptions lead to an increase in the optimum longevity.

5. As the interest rate approaches zero, the optimum value of  $k$  approaches the minimum of the objective average longevity cost curve,  $a(k)$ . Although the right-hand side of expression (3)—the marginal revenue curve—is not defined for  $r=0$ , its limiting value as  $r$  approaches zero is defined. We may evaluate this limit according to a well-known rule: Differentiate the numerator and denominator separately and evaluate the quotient at  $r=0$ . This gives us

$$\lim_{r \rightarrow 0} \frac{A r e^{-kr}}{1 - e^{-kr}} = \frac{\frac{\partial A r e^{-kr}}{\partial r}}{\frac{\partial (1 - e^{-kr})}{\partial r}} \bigg|_{r=0} = \frac{-A r e^{-kr} k + A e^{-kr}}{k e^{-kr}} \bigg|_{r=0} = \frac{A}{k} \equiv a(k).$$

As  $r$  approaches zero, our marginal revenue function approaches the objective average longevity cost function; therefore, at least cost,

$$A' = a(k).$$

Hence, as  $r$  approaches zero the optimum longevity approaches the minimum of the average longevity cost curve.

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15. United Nations, *Economic Survey of Europe in 1955*. Geneva 1956. (Citing Verein Deutscher Maschinenbau-Anstalten *Wirtschaftsbilder*, Aug. 1952.)

## "PEOPLE'S CAPITALISM" AND STOCK-OWNERSHIP

By VICTOR PERLO\*

It has become the fashion to classify the economy of the United States as a new form, "People's Capitalism." The expression was developed by the Advertising Council, which prepared a "People's Capitalism" exhibit, shown internationally under the auspices of the United States Information Agency.<sup>1</sup> The term is accepted by publicists and widely propagated in corporation reports and advertisements. It is used by some research and academic economists. Future editions of economic texts can hardly fail to discuss the theory—or slogan—of "People's Capitalism."

The central component of "People's Capitalism" is the contention that ownership of American industry has become democratic in character through the dispersion of stockholdings among the population.<sup>2</sup> This is not only given the most attention, but also is the feature that involves an alleged qualitative change in structure. The present paper is devoted to this theme.

There follow typical statements by professional economists, corporations and their officers. Marcus Nadler writes:

The economy of the United States is rapidly assuming the character of what may be termed 'People's Capitalism,' under which the production facilities of the nation—notably manufacturing—have come to be increasingly owned by people in the middle and lower income brackets or indirectly by mutual institutions which manage their savings.<sup>3</sup>

This is the principal one of the "striking political and social transformations" which Nadler states have taken place during the past three decades—that is since the late 1920's.

Roger M. Blough, chairman of the Board of U.S. Steel Corporation, states that our economic system has a "remarkable propensity—to divide the benefits of its own multiplying among those who contribute

\* The author is an economic consultant, in New York.

<sup>1</sup> *New York Times*, Feb. 14, 1956, p. 20; Sept. 3, 1956, p. 14.

<sup>2</sup> An exception was the American Round Table discussion of People's Capitalism, jointly sponsored by Yale University and The Advertising Council. While stressing the importance of widespread ownership, the panel members "agreed" that this "need not be regarded as the most essential part of a people's capitalism"—The American Round Table, *Discussions on People's Capitalism*, November, 1956, New York 1956, p. 8.

<sup>3</sup> M. Nadler, *People's Capitalism*, pamph., New York 1956, p. 5.

to it."<sup>4</sup> According to Blough, industrial executives since the depression of the 1930's have sought consciously to foster this division of benefits. He cites as a significant example:

... the change that has occurred in the ownership of our larger enterprises. Today fewer businesses—especially our biggest businesses—are owned by a few wealthy individuals or groups, as many were back in the Nineties. They are owned by millions of people in all walks of life. In United States Steel, for example, the owners of our business outnumber the employees by a considerable margin; and no one of them holds as much as three-tenths of one per cent of the outstanding stock.<sup>5</sup>

The General Electric Corporation heads a full-page advertisement: "People's Capitalism: The 376,000 owners with savings invested in General Electric are typical of America, where nearly every citizen is a capitalist."<sup>6</sup>

Standard Oil Company (N.J.), tells its employees that Karl Marx:

... devised a theory. ... Ownership of the mills, as with ownership of the land, was the key to the future. Ownership should, therefore, be vested not in the hands of the few, but with something he identified as The People.

Today, says Standard Oil, this is realized in the United States:

Yes, the people own the tools of production. ... By his own definition, Karl Marx' prophecy has been realized. ... How odd to find that it is here, in the capitalism he reviled, that the promise of the tools has been fulfilled.<sup>7</sup>

A. D. H. Kaplan writes:

The number of stockholders now equals or exceeds the number of employees in many large American corporations. The effect of prevailing tax rates on inheritance and income is toward progressive diffusion of the personal capital holdings in American corporations.<sup>8</sup>

A secondary component of "People's Capitalism" is the contention that incomes have been redistributed from the rich to the poor. That has been presented most authoritatively by Simon Kuznets<sup>9</sup>; the opposite viewpoint is presented in my booklet, *The Income "Revolution."*<sup>10</sup>

An auxiliary feature is the high American living standard. This has

<sup>4</sup> R. M. Blough, address before the Economic Club of New York, Jan. 15, 1957, p. 6.

<sup>5</sup> *Ibid.*, p. 9.

<sup>6</sup> *Wall Street Jour.*, May 13, 1957.

<sup>7</sup> Esso Corp., *The Story of Creative Capital*, pamph., undated.

<sup>8</sup> A. D. H. Kaplan, *Big Enterprise in a Competitive System*, Washington 1954, pp. 178-79.

<sup>9</sup> S. Kuznets, *Shares of Upper Income Groups in Income and Savings*, New York 1953.

<sup>10</sup> New York 1954.

been taken over from earlier positive appraisals of the workings of capitalism in the United States, and involves little that is new in principle.

The conclusion of the analysis in this paper is that the main justification of the term "People's Capitalism"—widespread stock-ownership—is without substance.

### I. Trend in Number of Stockholders

Has there actually been a sharp rise, since 1929, in the number and proportion of the population owing stocks? Table I presents available estimates:

TABLE I.—PROPORTION OF STOCKHOLDERS TO POPULATION IN UNITED STATES 1927-56

Year	Number of Stockholders	Population (millions)	Stockholders as Per Cent of Population
1927 (a)	4-6 million	119	3.4-5.0
1927 (b)	5-6 million	119	4.2-5.0
1930	9-11 million	123	7.3-8.9
1937	8-9 million	129	6.2-7.0
1952	6,490,000	157	4.1
1954	7,500,000	162	4.6
1956	8,630,000	168	5.1

Sources: Population from U.S. Bureau of Census.

Number of stockholders:

1927 (a) A. A. Berle Jr. & G. C. Means, *The Modern Corporation and Private Property*, New York 1932, p. 374.

1927 (b) and 1930 N. R. Danielian and others, *The Security Markets*, New York 1935, pp. 49-50, 723 ff.

1937 Temporary National Economic Committee, Monogr. No. 29, Washington 1940, p. 168.

1952 L. H. Kimmel, *Share Ownership in the United States*, Washington 1952, p. 89.

1954 and 1956, New York Stock Exchange, shown in *Economic Report of the President*, January 1957, Washington 1957, Table D-21, p. 112.

The three postwar estimates are limited to stockholders in publicly owned corporations, whereas the attempt was made also to include in the prewar estimates stock-owners of small private family corporations. Kimmel estimated that in 1952 there were 3 million owners of privately held stocks, but commented: "These do not, of course, constitute net additions to the number of share owners, for many of them also own publicly held issues."<sup>11</sup> The description of the estimating methods used by the TNEC suggests that its figure for 1937 might have been lower by 1½-2 million if privately held corporations had been omitted. With this qualification in mind, the following comments based on Table I are in order:

<sup>11</sup> L. H. Kimmel, *Share Ownership in the United States*, Washington 1952, p. 126.

The percentage of the population owning stock in 1956 was, if anything, lower than the corresponding percentage for 1930, but higher than that for 1927. There is a cyclical movement in the percentage of the people owning stocks. It increased sharply during the culminating years of the bull market of the 1920's; declined during the subsequent depression, and increased again during the bull market of the middle 1950's. But the statistics do not establish a secular uptrend in the proportion of the population participating directly in ownership of American industry over the past three decades.

However, the percentages in Table I understate the dispersion of stock-ownership among economic units, because one stockholder often carries the stock for the benefit of his family. Kimmel estimates that the 6,490,000 stock-owners in 1952 were distributed among 4,750,000 family units, or 9.5 per cent of the 50,000,000 such units in the country.<sup>12</sup> The Michigan Survey Research Center estimated that 8 per cent of all spending units held stock in 1955.<sup>13</sup> Roughly speaking, we may say that about one out of ten families own stock—a significant proportion, but hardly large enough to justify assertions of the *general* participation of the population in ownership of the means of production.

Blough and Kaplan drew unwarranted conclusions concerning dispersion of stock-ownership from their observation that the numbers of stockholders in particular companies exceed the numbers of employees of the same companies. They failed to take account of ownership of stock in several corporations by the same person. Kimmel has estimated that there were about 30 million *shareholdings* in American corporations, or more than 4 per individual stock-owner.<sup>14</sup> The latest estimate of the total number of stockholders, 8,630,000, compares with 47,282,000 private employees in 1955,<sup>15</sup> of whom perhaps 35 million were employed by corporations.<sup>16</sup> Thus, while the number of corporate employees did not exceed the number of individual *shareholdings* in corporations by a wide margin, it equaled about 4 times the number of *stock-owners*.<sup>17</sup>

Stock-ownership remains very unevenly distributed among various occupational groups and income groups. Kimmel found the per cent of different groups owning stocks ranging downward from 44.8 per cent of administrative executives and 19.4 per cent of operating supervisory

<sup>12</sup> *Ibid.*, p. 97.

<sup>13</sup> *Fed. Res. Bull.*, June 1955, XLI, 612, Table 3.

<sup>14</sup> Kimmel, *op. cit.*, p. 122.

<sup>15</sup> *Surv. Curr. Bus.*, July 1956, XXXVI, 19, Table 26.

<sup>16</sup> Estimated on basis that 74.5 per cent of compensation of private employees is paid by corporations.

<sup>17</sup> References in this section are to stock-owners in publicly owned corporations, the type relevant to the issue under discussion. By setting up a family-owned corporation, a retail merchant becomes a stock-owner, but there is no real structural change involved.

officials to 1.4 per cent of semiskilled workers and 0.2 per cent of unskilled workers. Among propertied, managerial, and professional classes generally, 13.4 per cent were stock-owners, while among employee classes generally, 3.5 per cent were stock-owners.<sup>18</sup> Similarly, 24.7 per cent of people in families with incomes over \$10,000 owned stock, while only 1.4 per cent of those in families with incomes under \$4,000 owned stock. The majority of the population was in the latter group, but it included only one-fifth of the stock-owners.<sup>19</sup>

While exact comparisons are not possible, owing to changes in the purchasing power of the dollar, the proportion of lower-income individuals owning stock does not appear to be larger than estimated for 1927 by Berle and Means, or for 1937 by the TNEC.

A similar comparison is available for the number of employees owning stock in the companies for which they work. In 1927, according to National Industrial Conference Board figures cited by Berle and Means, 800,000 employees had become stock-owners in their employing companies.<sup>20</sup> Kimmel's estimate for 1952 was 780,000.<sup>21</sup> In view of the substantial increase in the number of corporate employees over the 25-year interval, it appears that there was a definite decline in employee stock-ownership. Presumably this resulted from the depression sale by workers under financial stress of stocks previously acquired under company stock-purchase plans. Since such plans have been revived recently, it is likely that the number of employee stock-owners has increased somewhat since 1952.

Kimmel's figures show 3.2 per cent of corporate employees owning shares in their employing companies. The percentage is much higher in specialized types of corporations, notably public utilities and financial companies. For manufacturing, where two-thirds of the corporate employees are to be found, it is only 1.4 per cent.<sup>22</sup>

Thus, a substantial frequency of stock-ownership appears only in those occupational classes and income groups customarily associated with property ownership. Stock-ownership is still occasional, rather than typical, for workers, and rare for industrial workers. All but a handful remain separated from ownership of means of production.

## II. Concentration of Stock-Ownership

The concept of an economic democracy based on stock-ownership requires not only that there be a large number of owners, but that a

<sup>18</sup> Kimmel, *op. cit.*, p. 98.

<sup>19</sup> *Ibid.*, p. 97.

<sup>20</sup> A. A. Berle, Jr. and G. C. Means, *The Modern Corporation and Private Property*, New York 1932, p. 59.

<sup>21</sup> Kimmel, *op. cit.*, p. 113.

<sup>22</sup> *Loc. cit.*



large number own enough shares to have economic significance. It also requires that the situation should not be one in which a small number of stock-owners, by virtue of dominant holdings, are in a position of partial or total control.

A man owning a single share, or even 10 to 20 shares, of a typical industrial corporation, obviously has but a token stake in the ownership of the means of production. Consider the man who has invested \$1,000 in corporation stocks. At recent yields, he might expect dividends of \$40 per year. This is equal to about 2 days' wages for the workers in such industries as steel and automobiles. Even with allowance for possible capital appreciation, the return will not provide a significant addition to his living standard, nor represent a major degree of profit participation. After-tax profits of manufacturing corporations, in 1955, amounted to \$702 per manufacturing employee, or  $17\frac{1}{2}$  times the dividend receipts of the \$1,000 investor.<sup>23</sup> The \$1,000 is a similarly small fraction of the total capital invested per worker in basic industries.

This represents, in fact, the typical situation of the comparatively small number of workers owning stock. The 1955 Survey of Consumer Finances reported 3 per cent of spending units headed by skilled and semiskilled workers owning some stock. The median amount owned was between \$500 and \$999. None covered in the survey (or too few to be recorded in the percentage table), had as much as \$5,000 in stock. The figures were identical for unskilled workers. In the case of clerical and sales workers, 9 per cent held stocks, and the median holding was a little over \$1,000. All of those reported as holding over \$25,000 in stock were in the managerial, propertied, and professional groups.<sup>24</sup>

The Survey of Consumer Finances would indicate stock-ownership by about three-quarters of a million spending units headed by wage earners (skilled, semiskilled and unskilled workers), the figure swelled to a certain extent by the inclusion of foremen's holdings. Assuming a mean stockholding of \$1,000, the total value of stocks held by all wage earners' families in the country came to something like \$750 million. That was equal to 0.3 per cent of the marketable supply of stock in the United States.

The TNEC compiled, as of 1937, the main stockholdings of some wealthy families. For example the du Pont family was estimated to have \$574 million in stock, the Rockefeller family \$397 million, and the Mellon family \$391 million.<sup>25</sup> Allowing only for publicly reported

<sup>23</sup> Computed from *Surv. Curr. Bus.*, July 1956, XXXVI, 17, Table 20, and XXXVI, 19, Table 25.

<sup>24</sup> *Fed. Res. Bull.*, June 1955, Suppl. XLI, 622, Table 19.

<sup>25</sup> TNEC, *Investigation of Concentration of Economic Power*, Monogr. No. 29, *The Distribution of Ownership in the 200 Largest Nonfinancial Corporations*, Washington 1940, Table 6, p. 116.

changes in these family holdings—and there is no evidence of their significant dispersal—by 1956 the value of holdings of the Rockefeller and Mellon families exceeded \$3 billion each, and of the du Pont family \$4 billion.<sup>26</sup>

In short, any one of these families—or more properly speaking, groups of related families—owned many times as much stock as all the wage earners in the United States. Indeed, the market value of Rockefeller holdings in a single corporation, Standard of New Jersey, was twice the market value of all the holdings of all American wage earners. Apparently the Corporation neglected to take this into account in preparing the pamphlet quoted above.

The distribution of shareholdings in Standard Oil (New Jersey) is revealing. As of 1938 the 100 largest stockholders of record had 12,584,000 shares, or 46.2 per cent of the total. Most of these were various holdings of the Rockefeller, Harkness, Payne, Pratt, Whitney, and a few other families.<sup>27</sup> On the other end of the scale, 103,626 stockholders, each with 100 shares or fewer, and comprising 79 per cent of all stockholders, had 2,302,000 shares, or 8.4 percent of the total.<sup>28</sup> While the number of stockholders has increased with the splitting of the stock, there is no reason to believe that there has been any material change in the distribution.

No stock is so widely dispersed as that of the American Telephone and Telegraph Co. But in 1937-39, the very small holders, with 1 to 10 shares each, numbered 358,000 and had only 9.5 per cent of the total stock; while the large holders with over 500 shares each numbered 2,478 and held 44.3 per cent of the stock.<sup>29</sup> Since the share of the 20 largest holders has increased since then, there is no reason to believe there has been any material reduction in this contrast.

The over-all picture of concentration of stock-ownership is equally striking. The staff of the Senate Committee on Banking and Currency deduced from the 1952 Survey Research Center report that 8 per cent of all stock-owners, comprising: "less than one per cent of all American families owned over four-fifths of all publicly held stocks owned by individuals."<sup>30</sup>

Butters, Thompson, and Bollinger made similar estimates as of 1949. They found that 50,000 spending units, or about one-tenth of one per

<sup>26</sup> V. Perlo, *The Empire of High Finance*, New York 1957, Table I, p. 45. Subsequent estimates by *Fortune*, Nov. 1957, LVI, 177, for individual members of these families are consistent with the cited figures.

<sup>27</sup> T.N.E.C., *Hearings*, Part 14-A, *Petroleum Industry*, Washington 1940, pp. 8031-34.

<sup>28</sup> T.N.E.C., Monogr. No. 29, *Basic Statistical Data*, Appendix III, Sheet 6, facing p. 242.

<sup>29</sup> *Ibid.*, Appendix III, Sheet 1, facing p. 242.

<sup>30</sup> Senate Committee on Banking and Currency, Staff Report, *Factors Affecting the Stock Market*, Washington 1955, p. 90.

cent of all spending units, owned over \$100,000 of stock each. Their combined holdings were estimated at 65-71 per cent of the total of marketable stock outstanding. On the other extreme, 2,470,000 spending units, more than half of the total owning stock, had less than \$1,000 each. Their combined holdings amounted to only 1 per cent of the total outstanding.<sup>31</sup>

These authors also estimated the distribution of stockholdings by family-income level. Naturally, the degree of concentration shown in this way was somewhat less, because not all of the largest stockholders are in the highest income group. Their minimum estimates of the concentration at the top were: 1 per cent of the spending units (with incomes over \$15,000), held 65 per cent of the stock; one-half of one per cent of the spending units (incomes over \$25,000) held slightly over 50 per cent of all stock; and one-tenth of one per cent of the spending units (incomes over \$50,000), held 35 per cent of all stock.<sup>32</sup>

Prewar studies showed a similar concentration. Kuznets showed that there has been a decline in the percentage of concentration of dividend receipts among the top 1 per cent of the population, from 71 per cent of total dividends in 1929 to 53 per cent in 1948.<sup>33</sup> However, this estimate is not adjusted for the effects of changes in the tax laws on the methods of individual income reporting. Butters and associates, taking this into account, made the estimate for 1949, already cited, of 65 per cent, not far below Kuznets' 71 per cent for 1929. Qualifications cited by Butters and associates, and the reasonable range of statistical error, could account for the entire difference. At any rate, it is evident that the concentration of stock-ownership among a comparatively few individuals and families remains exceedingly great. The important stock-owners are numbered in the hundreds of thousands, rather than the millions. And the decisive stock-owners are numbered in the tens of thousands.

This conclusion is further supported by the specialized data available concerning current concentration in stockholdings within individual corporations. Information is limited to public utility and railroad corporations required to submit to government agencies figures as to the holdings of the largest stockholders of record. The data show, for most railroad and communication companies, an increase in the proportion of stock owned by the 20 largest holders of record since 1937. The 10 largest holders of stock in electric power companies in 1954 generally held a smaller share than in 1937, owing to the forced distribution of

<sup>31</sup> J. K. Butters, L. E. Thompson, L. L. Bollinger, *Effects of Taxation, Investments by Individuals*, Boston 1953, Table XVI-3, p. 382. This is the authors' estimate by the "residual method" which they regard as the "most reliable" of two methods used.

<sup>32</sup> *Ibid.*, p. 440.

<sup>33</sup> S. Kuznets, *op. cit.*, Table 123, p. 646.

holding company shares under the Public Utility Holding Company Act. Examples, for the largest companies, are shown in Table II.

Study of comparable data for smaller railroad and power companies shows that the results of Table II are representative. Unfortunately, similar data are not available for industrial corporations for the postwar period. However, the special legal conditions which caused a decline in power-company stockholding concentration were not duplicated among industrial corporations. The slight increase in concentration in American Telephone and Telegraph shares is noteworthy, since this is the most popular stock of all.

TABLE II.—PERCENTAGE OF COMMON STOCK OWNED BY 20 LARGEST HOLDERS OF RECORD SPECIFIED CORPORATIONS,<sup>a</sup> 1937 AND 1954

	1937	1954
Communications		
American Tel. & Tel.	3.8	4.2
Western Union	12.9	24.1
Railroads		
Pennsylvania	6.1	19.2
New York Central	23.7	42.6
Southern Pacific	15.0	15.3
Power companies (largest 10 holders)		
Pacific Gas & Electric <sup>b</sup>	25.1	10.0
Consolidated Edison	12.9	8.9
Commonwealth Edison	18.2	7.7

<sup>a</sup> The railroads and power companies shown are the three largest. 1954 statistics for General Telephone, 2nd largest communications company, are not available.

<sup>b</sup> Holders of common and preferred combined.

Sources: 1937 from TNEC Monogr. No. 29. 1954 from reports to the Federal Communications Commission, Interstate Commerce Commission, and Federal Power Commission.

The evidence, limited as it is, certainly gives no support for the hypothesis that stock-ownership has become less concentrated than prior to the second world war. The largest holders of record for companies supplying such data are almost all financial institutions, and the domination of institutions among the largest holders is more marked than in 1937. This is consistent with the known fact that institutional holdings of stock have increased relatively as well as absolutely.

### III. Institutional Stockholdings

Goldsmith estimated that the share of financial intermediaries in total domestic stock outstanding increased from 7.9 per cent in 1900 to 14.2 per cent in 1929 and 23.6 per cent in 1949.<sup>34</sup> I. Friend estimated

<sup>34</sup> R. W. Goldsmith, *The Share of Financial Intermediaries in National Wealth and National Assets, 1900-1949*, New York 1954, Table 16, p. 69.

a further rise in the proportion of institutional holdings subsequently.<sup>35</sup> My estimate for 1954, on a basis not strictly comparable with Goldsmith's, is 33 per cent.<sup>36</sup> Nadler and others interpret the growth of institutional holdings as a further evidence of the widespread distribution of ownership of the means of production.

Nadler gives particular stress to stock-ownership by mutual institutions, apparently regarding it as a more important means by which the mass of the population participates in ownership of equity capital than by direct ownership. Examples he cites include life insurance companies, mutual savings banks, and mutual investment funds.<sup>37</sup> It is my contention, however, that the great bulk of financial-institution stockholdings are of a character that cannot by any stretch of the imagination be regarded as representing ownership by masses of the population; that institutional holdings, in fact, reinforce the extreme concentration of stock-ownership in the hands of a small minority in the upper-income brackets.

Table III shows the New York Stock Exchange estimate of distribution of institutional holdings of corporation stocks, as of the end of 1954. These figures are close to estimates prepared by the Securities and Exchange Commission. They compare with estimates of the total potential market supply of stocks of \$250 billion and \$268 billion.<sup>38</sup> Thus institutions accounted at the end of 1954 for about one-fourth of all stockholdings, strictly financial institutions accounting for 21-22 per cent.

Two facts stand out in Table III. The stock owned by institutions of a "mutual" character, in which large numbers of the general public have any degree of proprietary interest, is a small part of the total. Holdings of life insurance companies, most of which are mutual, mutual savings banks, mutual-fund investment companies, and pension funds total \$12,960 million. Close to half of this, or \$5,840 million, consists of the mutual-fund holdings. Since the stockholders in these are included in the estimated totals of direct stock-owners, they represent no addition to the numbers participating in ownership of the means of production. That leaves \$7,120 million as the combined stockholdings of mutual institutions through which many millions of people, not otherwise involved in equity ownership, have an indirect beneficial interest, however attenuated. This sum represents a little over one-tenth of all institutional stockholdings, and roughly 3 per cent of all outstanding stock.

<sup>35</sup> Senate Com. on Banking and Currency, *op. cit.*, Table 3, p. 92.

<sup>36</sup> Perlo, *op. cit.*, p. 62.

<sup>37</sup> Nadler, *op. cit.*, p. 12.

<sup>38</sup> Senate Com. on Banking and Currency, *op. cit.*, p. 88.

TABLE III.—ESTIMATED INSTITUTIONAL HOLDINGS OF EQUITY SECURITIES, 1954  
(million dollars)

Type of Institution	Market Value
<b>Financial institutions:</b>	
Bank-administered personal trust funds	\$37,800
Fire and casualty insurance companies	6,460
Open-end investment companies (mutual funds)	5,840
Life insurance companies	3,400
Closed-end investment companies	1,450
Mutual savings banks	620
Subtotal	\$55,570
<b>Other institutions:</b>	
Pension funds (noninsured)	3,100
College-university endowment funds	2,500
Foundations, religious, and other charitable organizations	5,100
Grand Total	\$66,270

Source: U.S. Senate, Committee on Banking and Currency, Staff Report, *Factors Affecting the Stock Market*, Washington 1955, Table 6, p. 96.

The other notable feature is the domination of institutional stockholdings by one type, the bank-administered personal trust funds. This situation has prevailed throughout the 20th century, although the extent of domination has fluctuated. The share of personal trust departments in total stockholdings has grown markedly: according to Goldsmith's figures, from 5.1 per cent in 1900 to 8.4 per cent in 1929 and 15.2 per cent in 1949.<sup>39</sup>

It is, however, doubtful whether the New York Stock Exchange estimate of 1954 holdings by trust departments, cited in Table III, is adequate. The periodical *Trusts and Estates*, which specializes in the study of trust departments of banks, estimated that stockholdings of the latter amounted at the end of 1954 to \$62.6 billion.<sup>40</sup> Using the

<sup>39</sup> Goldsmith, *op. cit.*, Table 16, p. 69.

<sup>40</sup> *Trusts and Estates*, Feb. 1956, XCV, 100. For direct comparison with the Stock Exchange estimates, the *Trusts and Estates* figure should be reduced by \$3.1 billion to take out the pension-fund holdings. The difficulty of making an accurate estimate of trust-department holdings of stock arises from the fragmentary character of published reports, compounded by the practice of carrying securities at ledger rather than market value. A large part of the securities held in trust have multiplied many times in value since their acquisition, and this is only reflected in the accounts when switches of securities are made, a practice which is comparatively rare because of tax considerations. The reinvestment of dividends and the addition of new trust accounts at current market values correct for only a minor part of this bias. While both the New York Stock Exchange and *Trusts and Estates* have estimated corrections of this type, it is doubtful whether even the higher estimate of *Trusts and Estates* reflects the entire market value of trust department stock holdings.

*Trusts and Estates* figures, the personal trust departments of banks hold close to one-fourth of all corporation stocks, and by either estimate, they hold more than all other types of institutional investors put together.

Are these massive holdings an evidence of "People's Capitalism," or of a still greater concentration of stockholdings in the hands of the very wealthy than measured by their nonfiduciary holdings alone? In 1954 trust departments of national banks administered 289,000 personal trust accounts with \$43.4 billion of assets.<sup>41</sup> The main concentration of personal trust business is in New York, and it is primarily handled by state banks, hence for the most part not included in the report of the Comptroller of the Currency. The Federal Reserve Bank of New York reported that in 1954, 83 banks in the 2nd Federal Reserve District handled 115,000 personal trust accounts with \$48 billion of assets.<sup>42</sup>

From these figures it is clear that the enormous holdings of stocks and other properties by trust departments represent but a few hundred thousand accounts, quite large in average size. Since a single individual is often the beneficiary of several trusts, and since there are often a number of individuals in the same family for whom separate trusts are established, the number of families beneficiaries of these trusts may be but a fraction of the number of accounts.<sup>43</sup>

Moreover, these largely coincide with the largest direct stockholders. Butters, Thompson, and Bollinger found that among a sample of active

Confirmation of the superior reliability of the *Trusts and Estates* estimate is provided by the latest annual Federal Reserve Board survey of common trust funds (groupings by banks of smaller trust accounts of many individuals). At the end of 1956, 60.5 per cent of the market value of these funds was in stocks (*Fed. Res. Bull.*, June 1957, XLIII, 623, Table 1). While the common trust funds account for only a small proportion of total bank fiduciary holdings, there is no reason to believe that the proportion of stocks is higher in the common trust funds than in the much larger and older individual trust accounts. Indeed, Richard B. Chapman, president of the Trust Division of the American Bankers Association, has said that the proportion of common stocks in many of the older trusts has increased to from 70 to 85 per cent (*Trusts and Estates*, Sept. 1956, XCV, 813). Reports of the Comptroller of the Currency, and of various state banking departments, show that the total assets of personal trust accounts, at book value, exceed \$100 billion. Obviously, the market value is much higher, and if stocks comprise anywhere near 60 per cent of the total, the *Trusts and Estates* estimate is justified, as a minimum.

<sup>41</sup> U. S. Treasury Department, *Annual Report of the Comptroller of the Currency, 1954*, Washington 1955, Table no. 17, pp. 121-22.

<sup>42</sup> Federal Reserve Bank of New York, *Mo. Rev. Credit and Bus. Conditions*, June 1955, XXXVII, p. 73.

<sup>43</sup> In recent years, there has been a move towards making bank trustee services available to smaller investors, through the use of common trust funds. By the end of 1956 there were 243 of these funds, holding securities worth slightly less than \$2 billion for 93,000 fiduciary accounts (*Fed. Res. Bull.*, June 1957, XLIII, 622, 625). Clearly, this fringe service is still trifling in the total picture of bank fiduciary activities, and does not involve a significant part of the population.



investors, the proportion who were beneficial owners of trusts increased from 3 per cent among those with less than \$25,000 of wealth to 48 per cent for those with over a million dollars of wealth. Millionaires with trusts had 47 per cent of their wealth in that form.<sup>44</sup>

Thus the effect of the main form of institutional stockholding is to *increase* the concentration of stock-ownership. Butters and associates recognized this, commenting in relation to their estimates of concentration in stock-ownership cited above: "These figures would all be several percentage points higher if stock managed by corporate trustees for individual beneficiaries were included in the total."<sup>45</sup>

The "several" percentage points may be quite substantial indeed. The authors' estimates of personal trust stockholdings preceded the publication of data by the New York Federal Reserve Bank revealing that the New York banks alone held as much in personal trust accounts as had previously been estimated for the entire country, and before the recent *Trusts and Estates* estimates of a higher value of personal trust stockholdings than others current.

#### IV. Influence in Corporate Affairs

Effective participation in the business life of the country requires not only the beneficial ownership of a corporation's stock, but also at least a minimum of influence in the affairs of the corporation. It is scarcely necessary to belabor the point that the millions of small stockholders, and even the hundreds of thousands of medium-sized stockholders, have not an iota of influence in corporate affairs. The great majority of small stockholders either ignore corporate business, or limit their participation to signing proxies sent to them by a committee of the Board of Directors. Even in those corporations with highly publicized annual meetings, only a tiny proportion of stockholders attend. In the case of General Electric, where the proportion is unusually high, it slightly exceeds one per cent.

Moreover, even if all of the small stockholders could combine their forces to influence corporate affairs—an unlikely event—their votes would be insufficient. The 94 per cent of all stockholders having \$25,000 of stock or less have only 15-18 per cent of the total stock, and hence of the total votes.<sup>46</sup> As for the 55 per cent owning under \$1,000 of stock, and with combined holdings of 1 per cent of the total, they are utterly powerless.

On the other hand, the 50,000 spending units with \$100,000 or more of stock, and two-thirds of the total stock, or part of these 50,000, are

<sup>44</sup> Butters, *et al.*, *op. cit.*, Tables XV-5, 6, pp. 362-63.

<sup>45</sup> *Ibid.*, p. 440.

<sup>46</sup> *Ibid.*, Table XVI-3, p. 382.

the group in a position to have genuine influence in corporate affairs. Of course, comparatively few of these 50,000 are stock-owners in any given corporation. But it is from among these few that the dominant forces arise.

In 1937-39, the 20 largest stockholders of record in each of the 200 largest nonfinancial corporations owned altogether 31.6 per cent of the common stock and 30.47 per cent of the preferred stock.<sup>47</sup> In practical terms, these 20 very large holders, voting typically about one-third of the stock, exercise full effective voting control of the corporation. Thus Samuelson writes of large corporations: "The largest single minority ownership groups typically hold only about a fifth of all voting stock. Such a small fraction has been deemed more than enough to maintain 'working control'."<sup>48</sup> Reports of proxy contests during recent years make it clear that invariably both groups are led by those among the very largest holders. In short, contests are not between the small group of very large stockholders and the mass of small stockholders, but between rival groups of giant stockholders.

Concentration of control is furthered by the role of institutional stockholders, particularly the large trustee banks. Just nine New York City banks handled four-fifths of the city's personal trust business in 1954, and hence perhaps two-fifths of the national total.<sup>49</sup> These New York banks appear again and again among the 20 largest stockholders of record in the country's largest corporations in the prewar TNEC tabulations. The National City Bank appears in 8 of the 10 largest nonfinancial corporations. The Hanover bank, which published Nadler's pamphlet, appeared among the leading stockholders in 6 of the 10 largest nonfinancial corporations.<sup>50</sup> The resulting influence is expressed, among other ways, by particularly frequent representations on boards of directors. In 1955 five New York banks, among the leaders in trust business, each had interlocking directorates with scores of corporations having combined assets ranging from \$45 billion to \$70 billion.<sup>51</sup>

One can search far and not find a real representative of small stockholders among the directors of large corporations, whether a representative is defined as one having an occupation similar to that of the typical small stockholder (small business man, salaried employee, wage-earner), or being an official of an organization of any of these groups. Nor has there been any real change since the 1930's. Individuals have died off or retired, but the interests represented on the boards of

<sup>47</sup> T.N.E.C. Monogr. No. 29, Appendix IX, Tables 93, 94, pp. 601-2.

<sup>48</sup> P. A. Samuelson, *Economics*, 3d ed., New York 1955, p. 89.

<sup>49</sup> Federal Reserve Bank of New York, *op. cit.*, p. 74 and Perlo, *op. cit.*, Table 6, p. 70.

<sup>50</sup> Perlo, *op. cit.*, Table 11, p. 94.

<sup>51</sup> *Ibid.*, Table 12, p. 95.

such corporations as American Telephone and Telegraph, General Electric, and most others are virtually the same as they were 20 years ago, except for certain changes in the distribution of influence as between particular financial houses (as in the case of A.T. & T.).<sup>52</sup> Legal requirements have reduced the financial directorates on the boards of electric power companies. However, the conclusions of the Senate committee investigating the Dixon-Yates contract suggest that here also the real system of control remains despite formal changes.<sup>53</sup>

#### V. Conclusion

The basic claim of "People's Capitalism," that the rank and file of the population are becoming owners of the means of production in American industry, is without foundation in fact. The widespread diffusion of this theory signifies only the effectiveness of organized propaganda.

<sup>52</sup> *Ibid.*, pp. 96-98, 102-106.

<sup>53</sup> Senate Committee on the Judiciary, *Monopoly in the Power Industry*, interim report, Washington 1955, pp. 6-7.

## THE PRODUCTION CEILING AND THE TURNING-POINT OF 1920

By KENNETH D. ROOSE\*

A primary objective of this article is to examine the thesis that a production ceiling plays a crucial role in reversing the direction of economic activity at the top of the business cycle. It seems appropriate to investigate the relationship of a production ceiling to the turning-point of 1920 because demand forces were extremely strong and sharp price rises also occurred.

The basic argument is that the production ceiling is normally encountered first in the investment-goods industries. Thereafter further growth in demand for consumption-goods output depends largely upon lagged expenditures generated by the multiplier. Since these do not generate sufficient consumption, however, induced investment falls off and shortly consumption also declines.<sup>1</sup> This explanation of the causal factors in an economic turning-point seems unsatisfactory for 1919-20, because production of consumer goods reached its peak in November 1919, before that of investment goods, and began to decline markedly in February 1920, although prices and real consumption expenditures were still rising. Moreover, demand for investment goods, as reflected in new and unfilled orders, in many industries continued to rise for some months after production of consumption goods began to decline.

The data appear consistent, however, with a second type of production ceiling which, according to Hicks, may occur on rare occasions, primarily after big wars. At such a time a production ceiling may be encountered first in consumption-goods industries, and lack of adequate capacity in these industries then leads to further increases in production in the investment-goods industries. Under such circumstances inflation generally sets in before the check to the growth in consumption is able to exercise its customary restraint on investment.<sup>2</sup>

### I. *Capacity, Price and Production Data Summarized*

There was considerable transitional unemployment in the months immediately following the end of the war. The National Bureau of

\* The author is professor of economics at Oberlin College. Most of the research for this article was undertaken as a faculty research fellow of the Social Science Research Council.

<sup>1</sup> J. R. Hicks, *A Contribution to the Theory of the Trade Cycle*, London 1950, pp. 128-32.

<sup>2</sup> Cf. *ibid.*, p. 134.

Economic Research places the trough in economic activity in April 1919. Thereafter industrial production and prices rose sharply, the former reaching a seasonally adjusted peak in January and February 1920, the latter in May 1920. The peak for the reference cycle of the National Bureau of Economic Research was in January 1920. The decline which followed was one of the sharpest on record. Industrial production fell by almost one-third while wholesale prices fell by 45.3 per cent, or by more than their decline from 1929 to 1933. The reference-cycle trough was reached in July 1921.

Although economic developments in 1919-20 lend support to the less common version of a production ceiling, a critical question is whether production of consumption goods actually attained levels at which further expansion was limited by ceiling factors. This would appear to have been so, judged by the high levels of consumer-goods production. It seems much more likely, however, that individual industries rather than consumption goods in general were limited by capacity factors. Therefore as a major part of this analysis, sectional ceilings for individual consumption-goods and investment-goods industries were examined.<sup>3</sup>

Because of the difficulties in defining and especially in measuring capacity, it seemed best to rely on indirect evidence as to capacity levels of output. Since loss in production from seasonal factors might be measured by calculating the average deviations from the peak seasonally unadjusted rate of production, such a method might also indicate the deviations from latent capacity.<sup>4</sup> One characteristic of periods during which capacity has been strained is that the seasonal factor tends to drop out.<sup>5</sup> Hence when the deviation from the peak seasonally unadjusted rate of production is at a minimum, and there are other symptoms of maximum output such as price rises and reports of heavy demand and shortages, this may be taken as evidence that production is approaching a capacity ceiling. On the basis of this hypothesis, the seasonally unadjusted industrial production indices of the Board of Governors of the Federal Reserve System were examined for seven periods when it might be presumed that resources were relatively fully employed either generally or in selected areas: 1919-20, 1923, 1926, 1928-29, 1946-48, 1950-51, and 1952-53. For each period the average deviation from capacity was computed for the 6 consecutive months closest to capacity, which was defined as the monthly peak in the seasonally unadjusted production data.<sup>6</sup>

<sup>3</sup> Cf. *ibid.*, p. 132.

<sup>4</sup> Simon Kuznets, *Seasonal Variations in Industry and Trade*, New York 1933, p. 348.

<sup>5</sup> This observation was made to me by Moses Abramovitz of Stanford University.

<sup>6</sup> In a few instances the 6-month period does not include the peak production month from which the average deviation is computed.

The results of this analysis are quite suggestive. Of the 30 production series so examined, auto factory sales,<sup>7</sup> cigar, paper and pulp, and furniture production had their minimum average deviations from capacity in 1919-20 (when compared with the other 6 periods); each was a consumption good. In addition to these 4 series, 3 other consumption series, shoe, wheat flour, and newsprint production<sup>8</sup> were at their next-to-lowest average deviation. However, no investment-good industry had its minimum average deviation from capacity in this period.<sup>9</sup>

Statistics on prices and production of individual commodities also point to early pressure on resource use in consumption. By the fall of 1919, many more prices of consumer goods than of investment goods had exceeded their earlier peaks. Likewise, by December 1919, more than one-half of the series on consumer-goods production were at peaks for the 1919-20 period compared with just over one-tenth of the series on investment-goods production.

Production and price developments support the general conclusion that output in 1919-20 was limited by capacity in a number of consumption-goods industries. Their orders placed heavy demands on the investment-goods industries which continued to expand. New orders for investment goods reached peaks in the winter months but the backlog of orders continued to build up because of capacity limitations, because of speculative price rises which masked the fact that real consumer demand was declining, and because of the railroad strike in April 1920 which added to the confused picture of supply. Economic developments in 1919-20 thus appear to conform to the variant of Hicks' model in which an encounter with the production ceiling in the consumption-goods industries sets off inflationary price rises accompanied by a boom and price rises in the investment goods industries as well.

It does not follow, however, that the turning point of 1920 resulted *primarily* from the limitations on output imposed by production ceilings. In fact, a number of relatively autonomous factors also exerted deflationary pressures on the economy: (1) construction declined early in the period, residential construction contracts awarded reaching a peak in July 1919; (2) the commodity net balance of foreign trade began to decline in the middle of 1919; (3) the budget moved from a deficit to a surplus during 1919; and (4) monetary policy was tightened in November 1919 and was made more restrictive through the winter and spring of 1920. Taken together these factors reversed the inflation-

<sup>7</sup> If curtailed production of April 1920 (the month of the railroad strike) is included in the 6-month period, auto factory sales shift into the next-to-lowest average deviation.

<sup>8</sup> A small component of paper and pulp production.

<sup>9</sup> Crude petroleum, an investment-good series, was at its lowest average deviation from capacity in 1927 and at its next-to-lowest average deviation in 1919-20 and 1952-53.

ary spiral of 1919-20 and precipitated the economy into the depression of 1920-21.

## II. *The Empirical Results in Greater Detail*

Over-all production of consumer goods appears to have neared capacity levels. The index of production of consumer goods<sup>10</sup> at 113 in November 1919 was as high as at any time in the 'twenties and 'thirties except for its level of 114 in December 1925 and November 1928. Moreover, the production of consumer nondurable goods at 109 in November 1919 was not exceeded in the 'twenties or 'thirties and was equaled only in October 1926. Even the level of consumer durable-goods production of 130 in November 1919 was not exceeded until December 1923. In the eight months from March to November 1919, production of consumer durable goods increased by 80 per cent.

Table I summarizes the results of calculating the average deviations from capacity for 30 production series. The 4 series which had their minimum average deviation from capacity in 1919-20 are listed first. These industries, not including furniture, had a weight of 14.7 per cent in the industrial production index in the 1919-20 period. In order to economize on space, data on the 1926 and 1950-51 period are excluded from the table. An industry was not included for a given period if the peak rate of production in that period fell short of the peak rate of production in the preceding period by more than 10 per cent. Thus newsprint production and bituminous coal in 1929, and beehive coke production in the prosperous 'twenties were excluded by this test. In the period following the second world war by-product and beehive coke were replaced by coke in the index of industrial production. Tin consumption was not available separately. In 1946-48 tobacco other than cigarettes was far below 1945 levels. In 1952-53, wool textile production, cane sugar meltings, and tobacco other than cigarettes failed to come within 10 per cent of previous peak rates of production. The decision to classify a series as investment or consumption good was arbitrary. If the predominant use is in heavy industrial production the series was regarded as investment good. If the product often ends in consumer use it was classified as consumption good. In two cases, petroleum refining and gasoline production, no assignment seemed possible so the series were not used.

The average deviation from the seasonally unadjusted peak is a highly deductive test of capacity and subject to an unknown amount of error. Production itself may be evened out over time thus reducing the average deviation. In other cases, stand-by capacity may not have

<sup>10</sup> Unpublished monthly data from the Federal Reserve Bank of New York. The index base 100 equals long-term trend. Data are seasonally adjusted.



TABLE I.—PERCENTAGE DEVIATION FROM CAPACITY\* OF INVESTMENT- AND CONSUMPTION-GOODS PRODUCTION SERIES FOR SELECTED PERIODS, 1919-53<sup>b</sup>

	1919-20	1923	1928-29	1946-48	1952-53
<b>Consumption Series</b>					
Auto factory sales	3.8 <sup>a</sup>	6.3	8.2	6.8	4.8
Cigar production	5.0	7.2	6.9	8.2	10.1
Paper and pulp	1.0	4.3	1.8	2.8	2.0
Furniture <sup>d</sup>	0.6	1.8	7.7	2.0	2.0
Newsprint production <sup>e</sup>	1.4	5.0	— <sup>i</sup>	1.7	4.5
Shoe production	4.8	8.7	12.7	4.4	8.1
Wheat flour	7.1	16.9	9.1	7.0	3.6 <sup>m</sup>
Kerosene	5.1	4.5	4.1	9.2	8.8
Cotton consumption	4.0	3.6	3.4	4.3	3.5
Wool textile	3.7	3.3	2.6	1.9	— <sup>j</sup>
Leather tanning	4.8	3.4	4.5	11.8	4.6
Cane sugar meltings	8.9	3.8	7.2	13.2	— <sup>j</sup>
Meat packing	16.1	9.1	13.6	13.3	10.5
Cigarette production	13.8	3.0	4.7	9.2	8.0
Other tobacco	6.4	5.4	5.5	— <sup>j</sup>	— <sup>j</sup>
Newsprint consumption	6.7	6.3	5.8	6.6	6.5
Anthracite	7.2	5.5	14.2	3.5	19.2
<b>Investment Series</b>					
Pig iron production	6.7 <sup>c</sup>	5.0	2.7	2.1	2.0
Open hearth steel	5.8 <sup>a</sup>	5.7	3.8	3.0	2.3
Tin consumption	11.5	13.3	4.3	— <sup>k</sup>	— <sup>k</sup>
Lumber production	5.4	1.9	2.3	5.0	5.0
Stone, clay, glass production	5.5	3.1	4.0	3.6	2.3
Cement <sup>f</sup>	7.9	3.8	4.3	2.3	2.4
Lubricating oil	5.1	4.4	4.5	3.5	3.4
Fuel oil	2.6	— <sup>h</sup>	0.9	4.4	2.0
By-product coke	7.9	1.7	1.0	{ 0.9 <sup>l</sup>	{ 0.4 <sup>l</sup>
Beehive coke	7.5	— <sup>i</sup>	— <sup>i</sup>		
Bituminous coal	6.7	6.3	— <sup>i</sup>	5.6	16.2
Crude petroleum	1.2 <sup>a</sup>	1.5	3.8	2.4	1.2
Zinc production	5.6	2.9	2.9	11.1	2.1

\* Average deviation of six consecutive months that depart least from the peak monthly rate in seasonally unadjusted production.

<sup>b</sup> 1926 and 1950-51 excluded for space reasons.

<sup>c</sup> April 1920 excluded because of railroad strike but additional month added.

<sup>d</sup> Federal Reserve Bank of New York unpublished data for period before second world war.

<sup>e</sup> Component of paper and pulp.

<sup>f</sup> Component of stone, clay, glass.

<sup>g</sup> Lowest average deviation in 1927.

<sup>h</sup> Rising throughout the period.

<sup>i</sup> Peak more than 10 per cent below peak in preceding period.

<sup>j</sup> Far below 1945 levels.

<sup>k</sup> Not available.

<sup>l</sup> Replaced by coke.

<sup>m</sup> But almost 30 per cent below peak of 1946.

Source: Board of Governors of the Federal Reserve System, *Federal Reserve Index of Industrial Production*, Oct. 1943; *Fed. Res. Bull.*, selected monthly issues.

been brought into production even at high levels of output. Supply shortages for seasonal reasons or as a consequence of major strikes in the coal, steel, and railway industries may also have held output below true capacity levels in particular areas. Nevertheless, current accounts in 1919-20 stressed the extreme shortage of goods, and data on use of capacity obtained from various industry trade associations show a high degree of plant utilization in areas such as paper and board and newsprint production.

### *Timing of Peaks in Production and Prices*

The first large rise in wholesale prices was in July 1919, one month after the first major rise in industrial production.<sup>11</sup> The second large rise in wholesale prices was from November 1919 to January 1920 while industrial production also rose rapidly from December 1919 to January 1920. This second price rise, occurring even as capacity problems beset the consumption-goods industries, placed further pressure on production in the investment-goods industries. Total industrial production was at a peak in March 1920<sup>12</sup> but was followed by the third and final large price rise from March to April 1920.

Peak levels of output were attained earlier in the consumption-goods industries. Production series for consumer goods reached their peaks a number of months before those of the investment-goods industries. Thus seasonally unadjusted data (presented in Table II) show that 54 per cent (15 of 28) of a sample of consumption-goods series were at peaks by December 1919, but only 11 per cent (2 of 18) of the investment-goods series; in May 1920, 79 per cent (22 of 28) of the consumption-goods series had reached peaks compared with only 44 per cent (8 of 18) of the investment-goods series.<sup>13</sup>

The period of peak production for automobiles was from February through August 1920 and for paper and pulp from April through September 1920. As these were the two most important series with minimum average deviations from capacity in 1919-20, the timing of their peak periods of production might appear to invalidate the thesis that capacity problems emerged first in the consumption-goods industries. However, paper and pulp production had risen to within 10 per cent of capacity by October 1919 while automobile factory sales had done so by November 1919. Moreover, all of the consumption series listed in Table I had risen to within 10 per cent of their peak rates of production by December 1919 compared with fewer than one-half of

<sup>11</sup> Seasonally unadjusted data.

<sup>12</sup> Durable goods production, seasonally adjusted, moved to a belated peak in August 1920.

<sup>13</sup> Approximately the same time pattern may be observed in seasonally adjusted data.

TABLE II.—TIMING OF PEAKS IN 46 SEASONALLY UNADJUSTED PRODUCTION SERIES, 1919-20

Consumption-Goods Series	Cumulative Total	Cumulative Percentage	Date of Peak	Investment-Goods Series	Cumulative Total	Cumulative Percentage
Leather tanning	1	4	June '19		0	0
Lavatory shipments } Sink shipments } Small wares shipments }	4	14	July '19		0	0
Other tobacco } Cigar }	6	21	Sept. '19	Stone, clay and glass	1	6
Set of cards	7	25	Oct. '19	Lumber	2	11
Newsprint consumption } Wheat flour } Cigarette } Worsted spindles }	11	40	Nov. '19		2	11
Combs } Kerosene } Furniture } Fine cotton goods }	15	54	Dec. '19		2	11
Meat packing } Cotton consumption } Woolen spindles }	18	64	Jan. '20	{Beehive coke } {Tin consumption } {Copper }	5	28
Wool, wide loom } Wool, narrow loom }	20	71	Mar. '20	{Pig iron } {Open hearth steel } {Zinc }	8	44
Carpet and rugs	21	75	Apr. '20		8	44
Shoe	22	79	May '20		8	44
Auto } Newsprint }	24	86	June '20	{Lubricating oil } {Woodworking machinery } {Petroleum wax }	11	61
Cane sugar meltings	25	89	July '20		11	61
	25	89	Aug. '20	Asphalt	12	67
Paper and pulp	26	93	Sept. '20	Fuel oil	13	72
Bathtub shipments	27	96	Oct. '20	{Cement } {By-product coke }	15	83
	27	96	Nov. '20	{Bituminous coal } {Industrial pumps }	16	89
	27	96	Dec. '20	Crude petroleum	18	100
Anthracite	28	100	Feb. '21		18	100

Source: Board of Governors of the Federal Reserve System, *Federal Reserve Index of Industrial Production*, Oct. 1943; Federal Reserve Bank of New York, unpublished data, and U. S. Department of Commerce, *Record Book of Business Statistics*, Suppl. *Surr. Curr. Bus.*, Pts. 1-3.

the investment series.<sup>14</sup> It seems likely, therefore, that these developments encouraged order-placing in the investment-goods industries.

If individual commodities are studied with respect to the timing of price rises,<sup>15</sup> 60 per cent (9 of 15) of the consumption-goods series had exceeded their previous peaks of 1918-19 or earlier by August 1919. Only 33 per cent (4 of 12) of the investment-goods series had experienced such a rise by that date. By January 1920, the turning point in

<sup>14</sup> Also a much larger percentage of consumption-goods series than investment-goods series had risen to within their average deviations from capacity by the beginning of 1920.

<sup>15</sup> The sample of price series is not identical to that of the production series.

the cycle, 73 per cent (11 of 15) of the consumption-goods series had exceeded their previous peaks but only 42 per cent (5 of 12) of the investment-goods series had done so. The prices of various building materials and foods behaved much more like those of the consumption-goods series than investment-goods series. One-half of the series on farm products had exceeded their earlier peaks by April 1919. In 3 of 6 cases, the building series had exceeded their earlier peaks by July 1919. These price developments are reviewed briefly in Table III.

TABLE III.—DATES IN 1919-20 BY WHICH PRICE SERIES EXCEEDED THEIR 1918-19 PEAKS\*

Date	Consumption Goods		Investment Goods		Food and Related Products		Building Materials	
	Cumulative Total	Cumulative <sup>b</sup> Per Cent	Cumulative Total	Cumulative <sup>c</sup> Per Cent	Cumulative Total	Cumulative <sup>d</sup> Per Cent	Cumulative Total	Cumulative Per Cent
June '19	6	40	3	25	3	50	2	33
Aug. '19	9	60	4	33	3	50	3	50
Jan. '20	11	73	5	42	4	67	4	67
Aug. '20	13	87	7	58	4	67	6	100

\* Two consumption series, 5 investment series, and 2 food and related products series never exceeded their 1918 or earlier peaks.

<sup>b</sup> Based on 15 series.

<sup>c</sup> Based on 12 series.

<sup>d</sup> Based on 6 series.

Source: U. S. Bureau of Labor Statistics, *Wholesale Prices, 1913 to 1927*, Bull. no. 473.

In summary, just as the production of consumer goods tended to reach capacity levels earlier so prices of consumer goods also rose rapidly before those of investment goods.

### *Expenditures on Consumer Goods and the Turning Point*

With the strain on resources apparently more acute and appearing first in consumption-goods production, it is surprising to find that production of consumer goods began to decline in December 1919 while prices and consumer spending were still rising. Contemporary accounts stress the importance of a "buyers' strike" in initiating this turning point, but data on consumer expenditures in current prices show expenditures rising into the late spring of 1920. However, data on the distribution to consumers<sup>16</sup> (mail order sales, department store sales, grocery chain store sales, variety chain store sales, and gasoline consumption) when reduced to constant prices show that real consumption expenditures rose very slowly after July 1919, increasing by less than 1 per cent to their peak in February 1920. Department

<sup>16</sup> Unpublished original data from the Federal Reserve Bank of New York but before elimination of trend. All series are seasonally adjusted.

store sales in real terms actually hit their peak in July 1919. It would appear, therefore, that as consumption-goods industries neared capacity levels in October-November, 1919<sup>17</sup> and tended to induce expansion of production in the investment-goods industries, real consumption expenditures continued to rise, but only slightly, to their peak in February 1920. Then even as consumption began to weaken, investment series neared peak levels. A major reason for the weakening in production of consumer goods, apart from scarcity of productive factors, may have been the growing inelasticity of price expectations.<sup>18</sup> From August 1919 to October 1919, wholesale prices of finished products and raw materials declined, while those of semimanufactures declined from August to September. Because of these price declines, manufacturers may have concluded that the rise in prices which commenced again in November 1919 would be of short duration.

#### *New and Unfilled Orders and Stocks*

A final bit of information relating to the timing of the 1920 turning point and the problem of capacity output comes from data on new and unfilled orders and stocks.<sup>19</sup> Among consumption goods there are only new-orders series, seasonally unadjusted, for bathtubs, lavatories, and sinks, each of which reached a peak in July 1919, small wares which was at a peak in December 1919, and fine cotton goods which was at its peak in April 1919. In general, seasonally unadjusted new orders for investment goods reached their peaks much later. In most instances, too, unfilled orders were at their peak in the spring or early summer of 1920 while stocks generally were at their minimum in the summer and early fall of 1920. Exceptions to this,<sup>20</sup> and consistent with the early strength in consumption, were the lows in department store stocks in January 1920 and in raw cotton stocks in August 1919.

<sup>17</sup> Rates of expansion in production of consumer goods slowed down appreciably in the fall of 1919 as peak levels of production were reached, which is consistent with the belief that capacity factors began to limit output.

<sup>18</sup> For comments on labor scarcity and inelastic price expectations in the period, see Thomas Wilson, *Fluctuations in Income and Employment*, London 1948, p. 103.

<sup>19</sup> Department store stocks are from the Board of Governors of the Federal Reserve System *Fed. Res. Bull.*, Dec. 1951; all other data are from U.S. Department of Commerce, *Record Book of Business Statistics*, Suppl. *Surv. Curr. Bus.*, Parts 1-3.

<sup>20</sup> Iron ore stocks also were at a low in April 1920, and gas and fuel oil stocks in March 1920.

## RICARDO AND THE 93% LABOR THEORY OF VALUE

By GEORGE J. STIGLER\*

Mr. Malthus shows that in fact the exchangeable value of commodities is not *exactly* proportioned to the labour which has been employed on them, which I not only admit now, but have never denied.

Ricardo, *Works*, II, 66.

Did Ricardo have a labor theory of value—did he believe that the relative values of commodities are governed *exclusively* by the relative quantities of labor necessary to produce them?

A considerable number of historians of economics have given a flat affirmative answer to this question—a surprisingly large number considering the fact that there is not the slightest basis for such an answer.<sup>1</sup> In the course of their expositions one encounters quite remarkable statements such as that Ricardo assumed that labor and capital were in a fixed proportion in all industries,<sup>2</sup> or that "Ricardo . . . constantly takes no notice of capital."<sup>3</sup> Presumably these writers did not have access to Ricardo's *Principles*.

More careful historians of doctrine have recognized the several and important departures from a pure labor theory that Ricardo emphatically made. There is, in fact, an almost continuous spectrum of interpretations. At one uninteresting extreme some writers argue that Ricardo simply forgot or did not understand the import of the qualifications he made. A very important group has advanced the view that Ricardo *wished* to hold a labor theory of value—Cannan heads his treatment: "Ricardo's Attempt to Revive the Pure Labour Theory."<sup>4</sup> They hold that under adverse criticism and honest self-examination, Ricardo was gradually forced to introduce in successive editions of the *Principles* a series of qualifications of increasing importance, so that in the end it

\*The author is professor of economics at Columbia University.

<sup>1</sup> Some examples are E. Whitaker, *A History of Economic Ideas*, New York 1940, pp. 422-25; P. C. Newman, *The Development of Economic Thought*, New York 1952, p. 85; Emile James, *Histoire sommaire de la pensée économique*, Paris 1955, pp. 88-89; and C. Gide and C. Rist, *A History of Economic Doctrines*, 2nd ed., New York, n.d., p. 164.

<sup>2</sup> G. Myrdal, *The Political Element in the Development of Economic Theory*, Cambridge 1954, p. 62; similarly W. Stark, *The History of Economics*, New York 1944, p. 36.

<sup>3</sup> J. K. Ingram, *A History of Political Economy*, New York 1897, p. 125.

<sup>4</sup> *A Review of Economic Theory*, London 1929, p. 172.

really was not a labor theory.<sup>8</sup> But Ricardo was not willing to abandon the theory completely: "his heart clung to the pure labour theory,"<sup>9</sup> or he made a "brave show" of "identifying, as far as might be, value and the amount of labour necessary for production."<sup>10</sup>

In the most sophisticated versions of this view, the formal exposition of Ricardo's theory is, as I believe, correct and complete in substance, but there persists a strong implication that Ricardo attributes more than quantitative importance to labor in determining values.<sup>8</sup> Thus, after carefully stating Ricardo's "modifications" of a labor theory, St. Clair says that Ricardo "sweeps them all into the waste-paper basket"; for "he never got rid entirely of the idea with which he started, namely, that labour is the only price exacted by Nature for her gifts."<sup>11</sup>

The only economists to argue at some length that Ricardo had a cost-of-production theory of value, so far as I know, have been Marshall, Diehl, and Viner—but who could wish for more comforting allies?<sup>12</sup>

The widespread misinterpretation of a leading doctrine of an economist of the first rank is not a product only of later viewpoints and knowledge, for it occurred already in Ricardo's lifetime. The present essay seeks to set forth precisely what Ricardo's theory of value was, and to examine the interpretation placed upon it by his leading contemporaries.

### I. Ricardo's Theory of Value

Ricardo's formulation of his theory of value was much influenced by his desire to correct what he believed to be the major errors of Adam Smith's theory. For Smith the long-run value of a commodity equaled

<sup>8</sup> This "evolution" in Ricardo's thought was apparently invented by Hollander, "The Development of Ricardo's Theory of Value," *Quart. Jour. Econ.*, 1903-4, XVIII, 455-91. Sraffa has recently shown that it rests upon a misconception; *Works and Correspondence of David Ricardo*, ed. Piero Sraffa, Cambridge, Eng. 1951, I, xxxvii ff. (Subsequent references to the latter will be given simply by volume and page numbers.)

<sup>9</sup> Cannan, *op. cit.*, p. 177.

<sup>10</sup> Alexander Gray, *The Development of Economic Doctrine*, New York 1931, p. 177.

<sup>11</sup> In addition to O. St. Clair, one may cite A. C. Whitaker, *History and Criticism of the Labor Theory of Value*, New York 1904, Ch. 5, p. 130-31, J. Schumpeter, *History of Economic Analysis*, New York 1954, pp. 590-95, and H. Biaujeaud, *Essai sur la théorie Ricardienne de la valeur*, Paris 1934.

<sup>12</sup> A Key to Ricardo, New York 1957, pp. 40, 348.

<sup>13</sup> Marshall's discussion is in the *Principles of Economics*, 8th ed. London 1920, Appendix I. It elicited from the leading historian of the English classical economics the remark that "Marshall endeavours to show, in defiance of all evidence, that Ricardo never desired to put forward the pure labour theory of value" (Cannan, *op. cit.*, p. 177n). Viner criticized Cannan's position in his distinguished review of Cannan's book, *Economica*, 1930, X, 78-80. Karl Diehl's extensive, but only moderately detailed, account is in *Sozialwissenschaftliche Erläuterungen zu David Ricardo's Grundgesetzen*, Leipzig 1905, Pt I, pp. 1-50.

At least two other historians of doctrine asserted what I take to be the correct view



its cost of production: the "natural" price of a commodity was the sum of the necessary payments for labor, capital, and land. A rise in the price of one of these factors, and in particular a rise in wages, would lead to a rise in the prices of the commodities in which the factor entered.<sup>11</sup> If the changes in values were to be more than nominal price-level changes,<sup>12</sup> however, this was clearly a superficial analysis: why should the relative values of commodities be affected in any systematic way by the absolute level of input prices? The organization, although not the content, of Ricardo's chapter on value can be interpreted as one which presses the criticism of Smith's theory to the utmost limits.

The analysis is limited to useful goods, produced in free competition, and the element of rent is temporarily put aside (and later shown not to enter into marginal cost). Ricardo begins with the simplest case: the commodities are produced by one type of labor alone, working perhaps on free land (I, 12 ff.) In this simplest case the relative values of commodities will clearly equal the relative quantities of labor necessary to produce them, and will be wholly unaffected by the absolute level of wages (no matter in what unit they are measured).

Consider next, with Ricardo, the case in which only labor is required to produce the commodities, but different types of labor are used in differing proportions (I, 20 ff.). The market will establish wage differentials corresponding to the differences in skill and training of the occupations, and "the scale, when once formed, is liable to little variation." Hence a rise of wages will affect the money costs of all commodities in equal proportion, and leave relative values unaffected. Ricardo did not consider the possibility that the relative amounts of skilled and unskilled labor employed to produce a commodity might change and hence its relative value would change; he could have asserted, however, that the relative value of the commodity will change only if the "common labor" equivalent of the original labor input changed.<sup>13</sup>

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but did not argue it: J. M. Ferguson, *Landmarks of Economic Thought*, 2nd ed., New York 1950, p. 106; and W. A. Scott, *The Development of Economics*, New York 1933, pp. 108-13.

<sup>11</sup> *Wealth of Nations*, Modern Library ed., Bk. I, Ch. 7. Ricardo quoted as a striking example of this argument the passage: "By regulating the money price of all the other parts of the rude produce of land, [the price of corn] regulates that of the materials of almost all manufactures. By regulating the money price of labour, it regulates that of manufacturing art and industry. And by regulating both, it regulates that of the complete manufacture. The money price of labour, and of every thing that is the produce either of land or labour, must necessarily either rise or fall in proportion to the money price of corn" (*ibid.*, p. 477).

<sup>12</sup> And Ricardo was not inclined to make this exception since he had a commodity theory of money.

<sup>13</sup> But a closer analysis would have indicated that the wages of superior labor contain interest on the investment in acquiring skill, and therefore the relative levels of wages and interest rates enter into relative values.

It is evident that we can still retain the proposition that relative values of commodities are independent of the absolute level of wages (and profits) if, when each worker is equipped with fixed capital, we assume that the ratio of fixed capital to labor is the same in every industry, provided the capitals have equal durability. And this is Ricardo's next case (I, 26 ff.). At this point it is not made clear whether the fixed capital earns a net return: Ricardo views the fixed capital as previously expended labor, and says, correctly but unnecessarily, that "exchangeable value of the commodities produced would be in proportion to the labour bestowed on their production; not on their immediate production only, but on all those implements or machines required to give effect to the particular labour to which they were applied."<sup>14</sup> At a later point it is made utterly clear that the contribution of the fixed capital consists of not only amortization quotas but also interest on the investment.<sup>15</sup>

And this is as far as Ricardo could go in attacking Smith's theory. The next step, and analytically the final step, is to allow the ratio of fixed capital to labor to vary between commodities, and when this is done: (I, 30):

This difference in the degree of durability of fixed capital, and this variety in the proportions in which the two sorts of capital [fixed and circulating] may be combined, introduce another cause, besides the greater or less quantity of labour necessary to produce commodities, for the variations in their relative values—this cause is the rise or fall in the value of labour.

The variations among commodities in the productive role of capital are classified as variations in (1) the ratio of fixed capital to labor (I, 34), (2) the durability of fixed capital (I, 31, 40), and (3) the rate of turnover of circulating capital (I, 37). A rise of wage rates relative to profit rates will lead to a relative rise in the values of commodities made with little fixed capital, or capital of short life, or with raw materials that turn over rapidly.<sup>16</sup>

This is of course a cost-of-production theory, and differs from Smith's theory only in the exclusion of rents from costs: "By cost of production I invariably mean wages and profits."<sup>17</sup>

<sup>14</sup> I, 24. Since the indirect ("hoarded") labor is in fixed proportion to the direct labor, exchangeable values are of course proportional to either part or the total.

<sup>15</sup> I, 39, where the case of a virtually perpetual asset is discussed.

<sup>16</sup> Ricardo's measure of value, a hypothetical product made by labor working with the average amount of capital, the capital being of average durability, and having an average "period of production," leads to the proposition that profits fall (measured in this unit) when wages rise, and the actual direction of movement of the values can be predicted; see my "The Ricardian Theory of Production and Distribution," *Jour. Pol Econ.*, June 1952, LX, 202-4.

<sup>17</sup> *Notes on Malthus*, Ricardo's *Works*, II, 42.

Ricardo believed that the changes brought about in the relative values of commodities by fluctuations in wages and profits were very small relative to those brought about by fluctuations in the quantity of labor (direct and indirect):

The reader, however, should remark, that this cause of the variation of commodities is comparatively slight in its effects. With such a rise of wages as should occasion a fall of one per cent. in profits, goods produced under the circumstances I have supposed, vary in relative value only one per cent.; they fall with so great a fall of profits from 6,050 l. to 5,995 l. The greatest effects which could be produced on the relative prices of these goods from a rise of wages, could not exceed 6 or 7 per cent.; for profits could not, probably, under any circumstances, admit of a greater general and permanent depression than to that amount.<sup>18</sup>

And so, although it would be "wrong wholly to omit the consideration of the effect produced by a rise or fall of labour [wages], it would be equally incorrect to attach much importance to it" and therefore in the remainder of the book he will "consider all the great variations which take place in the relative value of commodities to be produced by the greater or less quantity of labour which may be required from time to time to produce them" (I, 36-37).

I can find no basis for the belief that Ricardo had an *analytical* labor theory of value, for quantities of labor are *not* the only determinants of relative values. Such a theory would have to reduce all obstacles to production to expenditures of labor or assert the irrelevance or non-existence of nonlabor obstacles, and Ricardo does not embrace either view. On the other hand, there is no doubt that he held what may be called an *empirical* labor theory of value, that is, a theory that the relative quantities of labor required in production are the dominant determinants of relative values. Such an empirical proposition cannot be interpreted as an analytical theory, any more than the now popular view that the price level is governed by the wage level and the productivity of labor can possibly be defended as an analytical proposition.

This is not to say that Ricardo's analytical theory was correct, for it contained several important deficiencies. It excluded rent from costs, and even if the supply of land were fixed the rent a piece of land could yield in one use would be a cost to other uses. (Ricardo's practice of assuming that land was used to grow only corn obscured this point.) His theory was wrong in reducing all capital to previously expended labor plus interest; except in some irrelevant day of Genesis all capital has been made by the cooperation of earlier capital and labor and land. This view may have fostered his empirical judgment that labor

<sup>18</sup> I, 36. This passage underlies the title of this paper.

quantities were decisive, but one could have adopted (wisely or not) the empirical proposition even if he had a correct concept of capital. And of course if all commodities are not produced subject to constant costs, an explanation of relative values that ignores demand is simply inadequate.

## II. *The Interpretation by Contemporaries*

Ricardo's *Principles* received very diverse reviews, ranging from the adulation of McCulloch to the reaction of one anonymous reviewer that the volume "contains no valuable information in point of fact, and very little good reasoning in point of doctrine."<sup>19</sup> This same diversity extended to the interpretation of his theory of value.

J. B. Say could find only a simple labor theory of value. In the notes he added to the French translation of the *Principles*, he observed:

M. Ricardo does not appear to have included [in the contribution of machinery to the value of a commodity] the profits or the interest on the capital as constituent parts of the prices of commodities.

M. Ricardo . . . teaches throughout this book that the quantity of labor necessary to produce a product is the sole element of its price, . . .<sup>20</sup>

Malthus, the other leading economist of the period, did not attribute a labor theory to Ricardo, but chided him for his language:

If to this cause of variation [differences in rate of durability of capitals] we add the exception noticed by Mr. Ricardo, arising from the greater or less proportion of fixed capital employed in different commodities, the effects of which would shew themselves in a very early period of savage life; it must be allowed that the rule which declares "that commodities never vary in value unless a greater or less quantity of labour be bestowed on their production," cannot possibly, as stated by Mr. Ricardo, be "of universal application in the early stages of society."<sup>21</sup>

It should be noticed that apropos of this discussion, Ricardo says, "In all the observations of Mr. Malthus on this subject I most fully concur" (II, 58). The only difference between Malthus and Ricardo in their concepts of costs of production was that the former included and the latter excluded the rent of land. The real dispute between them

<sup>19</sup> *The British Critic*, N. S. VIII (1817), 354. The reviewer continued: "He holds, for example, and this is the leading principle of his system, that the price of all commodities brought to market, consists solely of the wages paid to workmen, and of the ordinary profits on the stock. . . ."

<sup>20</sup> *Des principes de l'économie politique et de l'impôt*, transl. by F. S. Constancio, Paris 1819, I, 28; II, 297.

<sup>21</sup> *Principles of Political Economy*, 1st ed., London 1820, p. 90; reprinted in *Notes on Malthus*, Ricardo's Works, II, 59.

centered on the proper measure of value, rather than on the determination of relative values. ||

When James Mill wrote his primer on the Ricardian economics, *Elements of Political Economy* (1821), he restated Ricardo's theory in substance. After stating a labor-quantity theory, he went on to explain at length that because of differences in capital-labor ratios in various industries the fluctuations in wages and profits affect exchange values. He concluded:

It is evident, however, that though this difference in the ratios according to which the wages of two kinds of labour were exchanged, and the different proportions in which they were applied in the production of commodities, would, upon a rise or fall in wages, alter the relative value of commodities, it would do so, without in the least affecting the truth of the previous proposition, that quantity of labour determined exchangeable value.<sup>22</sup>

The brazen illogic with which this passage closes was questioned by Ricardo: (IX, 127):

In page 76 there is a passage ending with these words "without in the least affecting the truth of the previous proposition," etc. etc. If a watch and a common Jack altered in relative value without any more or less labour being required for the production of either of them, could we say that the proposition "that quantity of labour determines exchangeable value" was universally true? What I call exceptions and modifications of the general rule you appear to me to say come under the general rule itself.

Mill forfeited all hope of entering the economist's heaven when, in the second edition, he retained the passage unchanged and then went on to compound the sin by turning the labor theory into a tautology: "If the wine which is put in the cellar is increased in value one-tenth by being kept a year, one-tenth more of labour may be correctly considered as having been expended upon it."<sup>23</sup>

Ricardo's other fervent disciple, McCulloch, treated him with greater kindness. He repeated Ricardo's analysis, and then, before embarking on his own argument that the increased value of wine or timber arising merely from the passage of time was due to labor, warned the reader:

But Mr. Ricardo was inclined to modify his grand principle, . . . so far as to allow that the additional exchangeable value that is sometimes given to commodities by keeping them after they have been purchased or produced, until they become fit to be used, was not to be considered as the

<sup>22</sup> *Elements*, p. 76.

<sup>23</sup> *Elements of Political Economy*, 2d ed., London 1824, pp. 97-98.

effect of labour, but as an equivalent for the profits the capital laid out on the commodities would have yielded had it been actually employed.<sup>24</sup>

The final disciple we shall notice is De Quincy. His exposition of the Ricardian theory took the form of a series of dialogues between himself and Philebus, an anti-Ricardian, and Phaedrus, a neutral. The debates went better, for De Quincy, than any in which I have ever been participant or spectator: De Quincy carried every point, no really embarrassing questions were posed to him; and his adversary capitulated handsomely after every sally.<sup>25</sup>

The dialogues were concerned with a defense of the proposition that a rise in general wage rates will not affect the relative values of commodities. Early in the discussion De Quincy asserts:

*The ground of the value of all things lies in the quantity (but mark well that word "quantity") of labour which produces them.* Here is that great principle which is the corner-stone of all tenable Political Economy; which granted or denied, all Political Economy stands or falls. Grant me this one principle, with a few square feet of the sea-shore to draw my diagrams upon, and I will undertake to deduce every other truth in the science.<sup>26</sup>

And again,

It is Mr. Ricardo's doctrine that no variation in either profits or wages can ever affect price; if wages rise or fall, the only consequence is that profits must fall or rise by the same sum; so again, if profits rise or fall, wages must fall or rise accordingly.<sup>27</sup>

The complications raised by different ratios of labor to capital in various industries are not considered.

There is reason for believing that De Quincy did not mean to attribute a simple labor-quantity theory to Ricardo, despite the explicit clarity with which this is asserted. The dialogues were never completed, and the complications may well have been postponed to these unwritten parts. In the later *Logic of Political Economy* (1844), De Quincy summarized the complications which Ricardo raised with respect to differing capital-labor ratios, and did not challenge their basic significance.<sup>28</sup> Yet the reader of the *Dialogues* would have received only the

<sup>24</sup> *Principles of Political Economy*, 1st ed., London 1825, p. 313. The qualifications arising out of differing capital-labor ratios are summarized on page 309.

<sup>25</sup> "Dialogues of the Three Templars on Political Economy," which appeared as three articles in the *London Magazine* in 1824; I use the reprint in *The Collected Works of Thomas De Quincy*, ed. by David Masson, London 1897, Vol. IX.

<sup>26</sup> *Ibid.*, p. 55; his italics.

<sup>27</sup> *Ibid.*, p. 60.

<sup>28</sup> Sec. VII; *Collected Works*, p. 196: "In this case, it can no longer be said that the prices of the resulting articles, according to the general rule of Ricardo, vary as the quantities of the producing labour: a disturbance of that law occurs."



account of the first approximation, in which capital (and various types of labor) are ignored.

Samuel Bailey's penetrating analysis of the value concepts of Ricardo and his contemporaries revealed with admirable clarity the carelessness, ambiguity, and dubious metaphysics that saturated this literature.<sup>29</sup> Yet this clarity was achieved partly by avoiding a real problem with which these economists were grappling: how can one measure the value of commodity A not merely in comparison with commodity B (the case Bailey studies) but in comparison with all other commodities? This latter problem, of which the isolation of monetary fluctuations is one instance, was surely the rationale of most of the discussion of a measure of value.

So far as Ricardo's theory of value is concerned, Bailey makes no charge that it is a labor-quantity theory.

Mr. Ricardo, indeed, explicitly allows the influence of other causes, such as time, differences in the proportion of fixed and circulating capital, and inequalities in the durability of capital, by which he admits the value of commodities is liable to be affected. Notwithstanding these modifications, however, his followers continue to lay down the position of quantity of labour being the sole cause of value in the most precise and positive terms; not that they deny the exceptions, but they appear to lose sight of their existence, and frequently fall into language incompatible with their admission; . . .<sup>30</sup>

It may be added that in substance Bailey accepts Ricardo's theory of value, including the exclusion of rent from costs of production.

We may recapitulate this brief survey. McCulloch, Bailey and Malthus correctly understood Ricardo's theory to be a cost-of-production theory excluding rent, and De Quincy should probably be added to this group. The theory was understood as a simple labor-quantity theory by Say and Mill, and also by Torrens.<sup>31</sup> It is worth repeating that Ricardo accepted Malthus' analysis and rejected Mill's. The theory was more widely understood in its correct sense in Ricardo's time than in later times.

### III. Conclusion

How did the misunderstanding of Ricardo's theory arise? Although Ricardo's exposition has been often and justly denounced, the main argument stands out clearly enough: it does not require great generos-

<sup>29</sup> *A Critical Dissertation on the Nature, Measures, and Causes of Value*, London 1825.

<sup>30</sup> *Ibid.*, pp. 230-31.

<sup>31</sup> "Mr. Ricardo has pushed this principle still further, and contended, that in all periods of society, whether before or after the accumulation of capital and appropriations of land, the labour expended upon production is the sole regulator of value." *An Essay on the Production of Wealth*, London 1821, p. vi.



ity or deep subtlety to comprehend the main structure of his value theory—indeed he has suffered from overly subtle reading. The confusion over his theory has arisen from more fundamental sources.

In Ricardo's period several factors probably played a minor role in the confusion. Ricardo's two leading disciples, Mill and McCulloch, asserted a labor-quantity theory with all emphasis, although actually neither held such a theory to the extent of denying that fluctuations in wage and profit rates affected commodity values. Their expositions naturally colored the interpretation of Ricardo, even though McCulloch expressly indicated his disagreement with Ricardo. Another source was the vast confusion of the causes of value with the proper measure of value, and in Ricardo's first edition a pure labor measure of value was used.

The main source of the confusion, however, was probably the failure of economists to distinguish clearly between analytical and empirical propositions. Among economists who were not methodologically self-conscious, who did not systematically consider the necessary and sufficient conditions for an equilibrium, the distinction would seldom be remarked. Ricardo's emphasis upon the quantitative importance of labor tended to be read as an analytical proposition that labour quantities were the sole regulators of value.<sup>22</sup>

The failure to distinguish between analytical and empirical propositions has been a source of much misunderstanding in economics. An analytical statement concerns functional relationships; an empirical statement takes account of the quantitative significance of the relationships. When Marshall viewed the demand for a commodity as a function of its price, the prices of closely related goods, and of income, he was criticized by members of the Walrasian school for failing to recognize that all prices in principle influence the demand for any commodity. This is a characteristic instance of the distinction in question: No Marshallian ever denied the existence of the formal relationships that were omitted; no Walrasian ever presented an empirical example of important error resulting from their neglect.

One further source of misunderstanding of Ricardo increased through time. His exposition was much influenced by his desire to refute what he deemed to be popular and pernicious fallacies, such as that a rise in wage rates increases all commodities' values, and that high money-wage rates lead to low profit rates. When these views dropped from sight the thrust of the chapter on value became more obscure, so the view

<sup>22</sup> The "philosophical" and "empirical" theories of value distinguished by Wieser and elaborated by A. C. Whitaker, *op. cit.*, bear only verbal similarity to the present distinction. In fact their "empirical" is my "analytical" theory, and their "philosophical" is either my "empirical" or a metaphysical theory of value.

could ultimately emerge that Ricardo was desperately trying to stave off for 20 pages the admission that labor requirements are not the only determinants of value.

Schumpeter asked why if Marshall's (and the present) interpretation of Ricardo is correct, there should have been any controversy—would it then not amount simply to the current cost-of-production theory?<sup>33</sup> One is inclined to reply that there was no controversy, and that the controversy was about something else. There was in fact no active controversy over the so-called labor theory in Ricardo's lifetime. The main points of controversy were different. First, Ricardo eliminated rent from the costs of production, which was not in keeping with popular views. Second, he appeared to deny (but did not do so) that supply and demand governed value; in fact he considered this a wholly superficial view that merely postponed analysis of the real determinants of relative values, namely the factors governing supply.<sup>34</sup> Finally, the endless dispute between Malthus and the Ricardians concerned the measure of value, not its causes.

The basic reason Ricardo's theory is often misinterpreted is that it was often misinterpreted in the past. If a theory once acquires an established meaning, each generation of economists bequeaths this meaning to the next, and it is almost impossible for a famous theory to get a fresh hearing.<sup>35</sup> Perhaps one hearing is all that a theory is entitled to, but one may plead that Ricardo deserves at least a rehearing—his theory is relatively more widely misunderstood today than it was in his lifetime. One can build a strong case that the modern economist need not be acquainted with Ricardo's work, but there is no case for his being acquainted with an imposter.

<sup>33</sup> *History of Economic Analysis*, New York 1954, p. 594.

<sup>34</sup> "It is admitted by everybody that demand and supply govern market price, but what is it [that] determines supply at a particular price? cost of production." *Notes on Malthus, Works*, II, 45.

<sup>35</sup> Very occasionally a theory, unlike a dog, has its second day, as when Keynes persuaded many economists of the error of the century-long tradition that Malthus' criticisms of the full employment assumption of Ricardo were invalid. The example is the more remarkable because the tradition was correct and Keynes was wrong.

## ADVANCES IN GAME THEORY

### A Review Article

By HARVEY M. WAGNER\*

What has happened to game theory since von Neumann and Morgenstern's monumental work first appeared<sup>1</sup>? Has it provided a tool with which to analyze neatly all problems of economic conflict? What misunderstandings and misapplications of game theory notions still persist? With commendable success, mathematicians R. D. Luce and H. Raiffa<sup>2</sup> furnish answers in a carefully written treatment of the subject. Their contribution serves both as an introduction to the rudiments of game theory and as a guide to the more esoteric facets of conflict analysis. Their treatment is only moderately technical, the more difficult parts being set aside in specially designated sections. Any economist interested in behavioral models of conflicts who has not kept abreast of game theory research over the last decade will certainly profit by reading this volume.

In the succeeding sections, we shall for the most part follow Luce and Raiffa's sequence of presentation; however, since they have chosen (e.g., p. 253, n.) not to comment on the specific connections between their exposition and applications to economic theory, we shall make such indications where appropriate. Section I will be devoted to the conceptual framework of game theory models and some of the principal difficulties which must be overcome in a general approach to conflict analysis. In Section II the utility assumptions which are crucial for a systematic study of risky situations will be considered; and in Section III the important distinction between utility and money in a model of economic conflict will be examined. Sections IV and V outline the elements of two-person and  $n$ -person games. Section VI briefly discusses those themes which are common to modern statistical decision theory, group decision processes, and game theory. In Section VII we note some of the likely future developments in game theory.

\*The author is assistant professor of statistics and industrial economics at Stanford University. He is grateful to P. A. Samuelson, R. Solow, R. Bishop and M. Dresher for earlier discussions concerning the relevance of game theory to economic analysis. This work was supported in part by an Office of Naval Research contract at Stanford University.

<sup>1</sup>J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior*, Princeton 1944 and 1947.

<sup>2</sup>*Games and Decisions: Introduction and Critical Survey*, by R. D. Luce and Howard Raiffa. (New York: John Wiley. 1957. Pp. xix, 509. \$8.75. It is also available from the Library of Science, \$5.95 to members.) All subsequent chapter and page references not otherwise identified are to this volume.

I. *The Game Theory Model*

Though there is no dearth of literature<sup>3</sup> on game theory, Luce and Raiffa are the first to state precisely in a language comprehensible to the non-mathematician all the axioms involved at each step of the analysis and to offer a critique of these assumptions.<sup>4</sup> Like any economic model, a "game" is an abstraction of real-life circumstances: Given the variables subject to each player's control, all possible eventualities in a conflict situation must be ascertainable. Nature may influence the game by operating chance devices at various stages. If Nature's probability distributions are known to the players, she assumes a dummy role; the potential outcomes are then specified in terms of the probabilities associated with each possible event. Otherwise, Nature is a bona fide player. Each opponent must choose from all the actions open to him, taking into account that his adversaries are doing the same thing.

As simple as the above characterization of a game may be, in this reviewer's opinion it seems to be one of the main stumbling blocks in an understanding of the theory. Luce and Raiffa carefully explain that a choice of action, or *pure strategy*, is not merely the decision to be made at a particular stage of play; rather, it is an entire prescription of what is to be done over the course of the game. For example, in a labor-management bargaining game consisting of a sequence of moves, a player's pure strategy is defined as one possible mode of behavior over the whole game. His strategy has allowed for every conceivable move that his opponent might make, and he has accordingly devised a counter move. Once both players have selected such comprehensive strategies, an umpire (in their absence) could match one choice of action against the other and thereby determine the outcome.

What are the drawbacks of this aspect of the model? As Luce and Raiffa indicate (pp. 7-8), one problem is that the full scope of potential outcomes cannot always be ascertained. Nature, for example, may have choices unimaginable to the players.<sup>5</sup> A second, more subtle difficulty takes on its full significance when we come to the notion of a utility function. Since a play encompasses the entire sequence of moves which are to be made, a fortiori the grand conflict is encountered only once. If the players envisage that they are

<sup>3</sup> For example, D. Blackwell and M. A. Girshick, *Theory of Games and Statistical Decisions*, New York 1954; H. W. Kuhn, A. W. Tucker, M. Dresher, and P. Wolfe, editors, *Contributions to the Theory of Games*, I, II, III, Princeton 1950, 1953, 1957; J. C. C. McKinsey, *Introduction to the Theory of Games*, New York 1952; S. Vajda, *The Theory of Games and Linear Programming*, London 1956; J. D. Williams, *The Compleat Strategist*, New York 1954. The last-cited book is not only exceptionally entertaining reading, but one of the few precise treatments available to the nonmathematical reader. Luce and Raiffa also cite an extensive bibliography.

<sup>4</sup> Luce and Raiffa devote Chapters 1, 2, and 3 to the fundamental assumptions which underlie the game theory approach to conflicts. These three chapters form a basis for all the special game situations which they subsequently consider.

<sup>5</sup> An illustration which occurs to us is that allocating funds to research and development (ranging from pure research to the drawing of final blueprints) may conceivably be such a game. The statistician's notion of a game against Nature, which is somewhat different, will be discussed in Section VI.

going to meet on the battlefield several times, then the supergame to be analyzed is the complete sequence of scrimmages.<sup>6</sup> We believe that economists have sometimes erred in their criticisms of game theory by not realizing that the game model by definition has already included time-sequence considerations. Because game theory purports to deal mainly with unique events, the usual notion of probability in terms of long-run frequencies must be modified. Finally, bringing in timing elements and admitting long sequences of moves may quickly render it impracticable to find all of a game's pure strategies; the amount of computation becomes overwhelming.<sup>7</sup> But economists in their behavioral models have never demanded that every contingency should be part of the structure. Luce and Raiffa therefore may rightfully ask their readers to examine patiently the attitudes of game theory, make any revisions which seem required for the purpose of advancing its development, and withhold final judgment until the theory has had sufficient chance to come to fruition.<sup>8</sup>

To keep the analysis within limits, they restrict themselves to games in which each player has a finite number of pure strategies.<sup>9</sup> In the two-person case, this assumption allows them to represent the conflict by the familiar game matrix (*i.e.*, by a table or rectangular array) in which the rows correspond to one player, the columns to the other, and the entry at the intersection of the *i*th row and *j*th column shows the outcome resulting from one player selecting his *i*th pure strategy and the other his *j*th.<sup>10</sup> In addition to these axioms dealing with the structure of the game, each player is postulated to have a utility function representing his ordering of the various outcomes and to know every other player's utility function.<sup>11</sup>

We feel that this extreme assumption is what makes the problem of selecting a strategy so difficult.<sup>12</sup> Furthermore, the assumption proves vexing to the

<sup>6</sup>J. Marschak, "Rational Behavior, Uncertain Prospects, and Measurable Utility," *Econometrica*, Apr. 1950, XVIII, 111-41. If we define bluffing as a move that results in a willful foregoing of utility in order potentially to "make a killing" later on, then bluffing may be a part of one's action *within* the scope of a strategy in a supergame. But bluffing with respect to the selection of the over-all strategy is inconsistent with the notion of selecting a strategy which attempts to maximize *ex ante* utility over the period of play.

<sup>7</sup>It is well known, for example, that chess is a "trivial game." The rules assure that any play will not last more than a certain finite number of moves. Hence *conceptually* one could list every strategy (*i.e.*, prescription of how to move for each configuration of the board) and then select the winning (or nonlosing) course of action. Obviously such a complete listing is not forthcoming in the foreseeable future.

<sup>8</sup>Pp. 10-11. They draw an analogy to developments in theoretical physics which have often taken decades to make an impact.

<sup>9</sup>With the exception of Appendix 7, which is devoted to games with infinite pure strategy sets, and Appendix 8 which surveys sequential compounding of two-person games.

<sup>10</sup>Recall that if Nature is a dummy player, the "outcome" is really a lottery or a gamble of prizes, each occurring with known probability.

<sup>11</sup>Luce and Raiffa only touch upon the provocative idea of a game in which each opponent has a misconceived notion of his adversaries' utility functions (pp. 269-74).

<sup>12</sup>In contrast, Cournot duopolists, who adopt a naive attitude toward their plight, merely have to solve a straightforward maximization problem to determine their strategies. Once educated to the same extent as game theory opponents, they readily realize that a reappraisal of their former strategies is due.

analyst because even though he can exploit the property that the players enjoy mutual omniscience as to preferences for gambles, he must be cautious not to make interpersonal utility comparisons (the origin and scale of the utility functions are arbitrary). In our view, this warranted reluctance to make such comparisons is undoubtedly an important reason why a mathematical theory of conflict must inevitably leave many vital questions unanswered.

The definition of rational behavior in conflict situations has been an incessant cause of controversy between the followers and disbelievers of the theory. As is so often the case, a good part of the dispute has been terminological rather than substantive. To set the record straight, we see no disagreement between economists and game theorists on what a player *should* do (in the sense of furthering his own well-being, as he sees it) when he believes he knows how his opponents will play. Under such circumstances the gaming aspect disappears. In duopoly theory discussions, economists have called such knowledge "conjectural variation," and the economist's duopolist accordingly maximizes profits. The game theorist refers to "a priori" information, and his player in an exactly analogous manner selects a "Bayes strategy" defined by these a priori beliefs.<sup>13</sup>

In a particular class of conflicts, viz., two-person zero-sum games (to be precisely defined below), believers in the theory unreservedly offer a prescription for an "optimal" strategy. We think it is regrettable that most dilettantes in game theory never become aware that game analysis has been extended beyond the two-person zero-sum case. No wonder they are disillusioned with the theory, for the usual conflicts in economics are not in this simple category; and what is more, the "optimal" strategy applied to nonzero-sum games is often a nonsensical mode of behavior. Game theorists are anything but in agreement as to what should be defined as "rational action" or an "optimal" strategy in the nonzero-sum case. Why there is agreement at least in one instance can best be appreciated after we have discussed the game theorist's view of utility constructs.

As a prelude to Luce and Raiffa's exposition, we have constructed the following simple two-person game to emphasize the difficulties which must be faced in the analysis of conflicts:

1. Each player privately selects his strategy, i.e., neither makes his choice known nor attempts to influence by bluffing, threatening, or similar tactics. An umpire receives the selected strategies from each participant and then announces the results.

2. Once each player has chosen his strategy, the final outcome is certain; this outcome may be a transfer of money, of goods, or a score, etc. Nature is not a party to the game, and neither player is sophisticated enough to envisage the possibility of randomizing (or using a probability device) in the selection of strategies.

<sup>13</sup> Loosely, a Bayes strategy is the selection of an action that maximizes "expected satisfaction," a concept which takes into account both the respective values of the possible outcomes and their alleged probability of occurrence. A more precise definition is given in Section II after we have defined a player's utility function for risky situations.

3. Each player has an ordinal utility function giving a consistent ranking of all definite outcomes which may arise in the game. As usual, such a utility function depends on each player's tastes.

4. If we list all possible outcomes (resulting from each pair of strategies) from best to worst as defined by the first player's utility function, then the same ordered list gives the outcomes from worst to best as defined by the second player's utility function.

5. When we describe the game by a pair of matrices whose entries are respectively player one's and player two's utilities of the outcomes, there is at least one saddle point in *both* matrices corresponding to some given pair of strategies.<sup>14</sup> The saddle point represents the pair of minimax strategies for the players.<sup>15</sup> Figure 1 illustrates the assumption. There may be more than one such saddle point. Note that the existence of a saddle point is not

		Player One's Utilities					Player Two's Utilities		
		Player Two's Strategies					Player Two's Strategies		
		$T_1$	$T_2$	$T_3$			$T_1$	$T_2$	$T_3$
Player One's Strategies	$S_1$	10	4	9	Player One's Strategies	$S_1$	0	5	2
	$S_2$	0	1	6		$S_2$	10	8	3
	$S_3$	9	0	3		$S_3$	2	10	7

Saddle Point:  $S_1, T_2$ .

FIGURE 1

changed if one makes a strictly monotonic transformation of either individual's utility function; thus a saddle point is independent of the particular form of the utility function employed.

6. It is known that the opponent is using a minimax strategy.

The saddle point, or equivalently the player's own minimax strategy, is easily seen to yield the best outcome under the given conditions, and consequently it seems reasonable to define the minimax selection as the optimal strategy in this case. The assumptions are clearly restrictive and only a relatively few games meet the hypotheses. But we may no longer unequivocally prescribe the minimax strategy if any one of our assumptions is significantly

<sup>14</sup> Given player one's utility matrix where his strategies correspond to the rows and player two's strategies correspond to the columns, a saddlepoint occurs where a numerical utility is both the minimum in its row and maximum in its column. Similarly, in terms of player two's utilities, the saddle point occurs where a numerical utility is both the maximum in its row and the minimum in its column.

<sup>15</sup> In order not to confuse our exposition by having to distinguish between one of the player's minimaxing, and the other's maximin, we shall use the term minimax to denote a general strategy: a player considers for each and every one of his strategies the utility from the worst consequence of his using that strategy; a minimax strategy is one which yields the maximum of these utilities of the unfavorable events. In this section, we restrict ourselves to the use of pure strategies only; we relax this condition below.



relaxed. Let us assume that five out of the six restrictions hold, but one of the restrictions is violated for a particular game.

1. If communication between the two players is allowed, then either one might very well attempt to influence the other by threatening, promising, or deceiving.

2. If probabilistic uncertainty, either in the outcome or in the selection of strategies, is introduced, we need to extend our utility function to include rankings for risky events.

3. If an individual does not have a complete and consistent (*i.e.*, transitive) ordering represented by a utility function, the notion of an optimal solution which maximizes utility is ill-defined.

4. If there is not a clear and definite conflict of interests in terms of utility of the outcomes for each player, it cannot be demonstrated that player one, say, should use his saddle-point strategy, even though player two chooses his minimax strategy.

5. If the game does not have a saddle point and player two, say, uses his minimax (pure) strategy, it is not generally advantageous for player one to use his minimax strategy.

6. Finally, if player one, say, knows that player two is *not* going to use his minimax strategy, it is usually not advantageous for player one to choose his own minimax strategy.

Hence, if a single restriction is removed, we shall need some further assumptions in order to state once more that minimax is an optimal procedure in a two-person conflict. When we extend our scope to more than two persons, further serious conceptual difficulties arise. In reviewing what progress has been made in analyzing these problems, Luce and Raiffa logically start with the underpinnings of the entire theory—viz., probability notions and utility theory.

## II. Utility Axioms for Stochastic Events

The stochastic or probability element enters game theory in the three places. First, given each opponent's pure strategy, the final outcome itself may be a random variable due to Nature's intervention. For example, the outcome of a pair of strategies might be to throw a die and to transfer an amount of money from one party to the other, the result depending on the particular random outcome of the throw. For another pair of strategies, a coin may be tossed, and some other monetary payoff is prescribed. Thus the entry in a game matrix can represent a "lottery" (a "gamble" or a "prospect" of random outcomes). It is erroneous to think of such an entry as always indicating a single nonrandom outcome. We can postulate extending the concept of a utility function to include an ordering of probabilistic situations. Then an entry in the actual game matrix for a player becomes the utility he attaches to getting the opportunity of playing the lottery. In the matrix characterization of a game, known as the *normal form*, two elements may have the same numerical utility value but may not imply that the same underlying event (a certainty or a lottery) will occur. The second place that probability may enter is in the case of a player choosing a randomized or mixed strategy. In other words,

the participant selects from his pure strategies according to some predetermined probability distribution. And finally, uncertainty may arise when the opponent's strategy is randomized according to a probability mixture which may be unknown to the player.<sup>16</sup>

As we have mentioned, long-run probability notions are not in accord with the single-play assumption. The "answer" to this conceptual problem lies mainly in the treatment of subjective probability. It is not a trivial problem to build a logical system on subjective probability, and here certainly is one of the controversial and difficult-to-comprehend aspects of game theory.<sup>17</sup> Most treatments of subjective probability are compatible with the usual numerical operations of probability theory. Thus, if the subjective probability of a head appearing upon the toss of a coin is  $\frac{1}{2}$ , the subjective probability of two heads appearing in two independent tosses is  $\frac{1}{4}$ .<sup>18</sup> Further, if an honest-to-goodness probability device is part of the mechanism of the game (e.g., numbered chips drawn from a bowl), subjective probability is assumed to agree with the usual objective definition of probability. The innovation of subjective probability is that it also applies to uncertain events which occur only once, and which perhaps are not even stochastic in orientation. For example, we sometimes speak of the probability of our taking a vacation within the next month, or of our getting a letter in the mail, or of our getting a raise in salary; it is unlikely in any of these illustrations that some human being is drawing a random number to determine the final outcome. Even in the case of coin-tossing we find it natural to say the *ex ante* probability of a given face is one-half, although we know that a single toss results *ex post* in either a head or a tail.

Adopting a humorous remark often made among statisticians in reference to the universal assumption of a normal distribution, we can say that mathematicians believe that economists have empirically verified the universal existence of a Bernoulli utility function,<sup>19</sup> and economists believe that mathe-

<sup>16</sup> A special case is when the opponent picks a single pure strategy, with probability equal to one, but the player does not know which pure strategy the opponent will select.

<sup>17</sup> Pp. 36, 300-3, Appendix 1. Several papers on this subject are contained in *Proceedings of the Second and Third Berkeley Symposia on Mathematical Statistics and Probability*, J. Neyman, editor, Berkeley 1951, 1956; F. O. Ramsey, *The Foundations of Mathematics and Other Logical Essays*, New York 1950; E. J. Savage, *The Foundations of Statistics*, New York 1954; G. L. S. Shackle, *Expectation in Economics*, Cambridge 1949.

One important assumption which is made throughout game theory is that whenever stochastic situations enter, every possible event has been enumerated, and that the events themselves are mutually exclusive. Therefore we can be sure that one and only one of the events considered will occur. Usually a finite set of possibilities is presented. Mathematically, it is possible to solve some game theory problems for an infinite set of outcomes; needless to say, the mathematical complexities increase.

<sup>18</sup> Shackle's axiomatics (*loc. cit.*) do not fulfill the usual "laws" of elementary probability.

<sup>19</sup> A (Daniel) Bernoulli utility function for uncertain events is defined as follows: Given the utilities for a set of nonrandom events, the utility for a lottery with these outcomes, each occurring according to a given probability distribution, is numerically equal to the expected value (or probability-weighted average) of the utilities for the certain events. Therefore Bernoulli utility over uncertain events implies a linearly additive function of the component utilities for each possible lottery "prize."

maticians have rigorously proved the function's universal existence. Luce and Raiffa give an excellent presentation of the von Neumann and Morgenstern type of approach to utility theory which yields a Bernoulli function for stochastic events.<sup>20</sup> We point out that perhaps one of the most misunderstood facets of game theory is the essentiality of assuming such a function. This is the tool enabling us to incorporate in the utility function a simple and precise method for evaluating *any* alternative stochastic situation of the sort just mentioned. The axioms needed are:<sup>21</sup>

*Axiom 1.* The individual has a preference ordering over all possible stochastic situations (nonrandom events being a special case). Given any two lotteries, the individual either strictly prefers one prospect to the other or is indifferent between the two.

*Axiom 2.* The individual's preference ordering is consistent. If, according to the tastes of the individual, lottery A is at least as good as lottery B, and lottery B is at least as good as lottery C, then the consistency condition asserts that lottery A must be at least as good as lottery C.

*Axiom 3.* If there are three lotteries, A, B, and C, such that the individual strictly prefers A to B, and B to C, then there is some "superlottery," or probability prospect of *both* A and C, with A heavily weighted, that is strictly preferred to B; and B is strictly preferred to some superlottery of *both* A and C, with C heavily weighted.<sup>22</sup>

*Axiom 4.* If lottery A is at least as good as lottery B, then *every* superlottery (or probability prospect) of A and C, where C can be *any* lottery, is at least as good as the *same* probability prospect of B and C.<sup>23</sup>

In recent years there have been various criticisms of the usual axioms of preference leading to a utility function, most of the debates centering around whether or not an individual can be assumed to possess a consistent ordering of all certain situations.<sup>24</sup> As Luce and Raiffa note, such criticisms, of course, can be made to apply to the generalized utility system for stochastic events, specifically to Axioms 1 and 2.

In addition to establishing a type of continuity of preference by using the intensity in a probability mixture as the independent variable, Axiom 3 denies

<sup>20</sup> Chapter 2. This chapter can be read independently of forerunning material, and in this reviewer's opinion it is one of the highlights of the book. Also see I. N. Herstein and J. Milnor, "An Axiomatic Approach to Measurable Utility," *Econometrica*, July 1953, XXI, 291-97; J. Marschak, *op. cit.*; D. Blackwell and M. A. Girshick, *op. cit.*, pp. 102-20.

<sup>21</sup> Our presentation differs insignificantly from that of Luce and Raiffa; to permit ease in exposition, we have sacrificed a slight amount of mathematical precision.

<sup>22</sup> An implication of the entire set of axioms is that there is some unique lottery of A and C such that the individual is indifferent to this prospect and to lottery B.

<sup>23</sup> This axiom has been named by Samuelson "the strong independence property" and by Savage "the sure-thing principle." P. A. Samuelson, "Probability, Utility and the Independence Axiom," *Econometrica*, Oct. 1952, XX, 670-78; L. J. Savage, *op. cit.*, p. 21.

<sup>24</sup> K. O. May, "Intransitivity, Utility and the Aggregation of Preference Patterns," *Econometrica*, Jan. 1945, XXII, 1-13; N. Georgescu-Roegen, "Choice, Expectations and Measurability," *Quart. Jour. Econ.*, Nov. 1954, XLVIII, 503-34; H. M. Wagner, "An Eclectic Approach to the Pure Theory of Consumer Behavior," *Econometrica*, Oct. 1956, XXIV, 451-66, and "Testing the Transitivity Axiom," *So. Econ. Jour.*, Apr. 1956, XXII, 493-94.

the existence of prospects offering Heavenly Bliss or Eternal Damnation. The individual's utility function is bounded. For example<sup>25</sup>, if the event C is to end in hell, and A and B are prizes of \$2.00 and \$1.00, respectively, then (contrary to the axiom) we might not be surprised to find an individual who prefers a dollar for sure, than any gamble between \$2.00 and hell, no matter how unlikely the latter possibility.

Some objections to the strong independence axiom have turned out to be due to a misunderstanding of its assertion.<sup>26</sup> To illustrate the meaning of the postulate, assume that prospect A is a three-to-one chance of getting two pounds of chocolates versus one quart of ice cream; prospect B, which by assumption is not better than A, is the four-to-one chance of receiving two quarts of ice cream or one pound of chocolates; and finally prospect C, which in fact may be any lottery we wish to consider, is assumed to be a one-to-one chance of getting two pounds of chocolates versus two quarts of ice cream. The axiom states that any lottery, say a five-to-one chance of getting A versus C, is at least as good as the lottery promising a five-to-one chance of B versus C. Why? The plausibility of the assumption is argued by recalling that in any case a lottery is to be played only once. For one superlottery, either prospect A will result or prospect C, but not both; and for the other superlottery, either prospect B will result or prospect C. Furthermore, whether A, B, or C results, an amount of chocolates or ice cream, but not both, will be the final outcome. Since the odds for getting C in either superlottery are the same, and the component events are mutually exclusive, the individual should be indifferent in this respect toward both lotteries. Since the probability of getting A or B is also the same for each superlottery, the fact that A is at least as good as B should imply that the individual finds the superlottery with A at least as good as the superlottery with B.<sup>27</sup>

The axiom states nothing about an individual's behavior for repeated choices of the lotteries, where he could presumably store and redistribute his consumption of the prizes over a period of time. Therefore the axiom does not apply to situations where intertemporal "contamination" or any other actual mixing of the prizes can take place. Only a single play is envisaged and only a single lottery prize results.

Wold<sup>28</sup> has raised the question that if we restrict ourselves to a single play of a game over a "relevant" horizon, such a formulation may render the axiom incapable of ever being refuted by observable behavior. According to this line of argument, any single decision of a person can be made rational upon properly defining the "relevant" horizon. Thus the complete acceptance of

<sup>25</sup> We have slightly altered an illustration given by R. M. Thrall, "Applications of Multi-Dimensional Utility Theory," in *Decision Processes*, Thrall, Coombs, and Davis, editors, New York 1954, p. 185.

<sup>26</sup> H. Wold, G. L. S. Shackle, and L. J. Savage, "Ordinal Preferences or Cardinal Utility," A. S. Manne, "The Strong Independence Assumption, Gasoline Blends and Probability Mixtures," A. Charnes, "Note," P. A. Samuelson, *op. cit.*, and E. Malinvaud, "Note on von Neumann-Morgenstern's Strong Independence Axiom," *Econometrica*, Oct. 1952, XX, 661-79.

<sup>27</sup> We use the term "should" with the meaning of "it seems reasonable to suppose."

<sup>28</sup> H. Wold, *op. cit.*, p. 664.

the sure-thing postulate, even when correctly defined, remains an issue among critics of game theory.

A further result can be derived from Axiom 4 if we consider A indifferent to B, and B and C being (in every sense) identical lotteries. Then the individual is indifferent between B and any probability mixture of A and B, thus disallowing for a "love of gambling" motive.

If we agree to the four axioms as a suitable formulation for behavior, and a fortiori the relevance of a Bernoulli utility indicator, we can easily handle the three types of uncertainty which we specified above. The fundamental utility theorem states that our hypothesized rational individual behaves as if he maximizes expected utility for the prizes treated as certainties. Hence, once a pair of strategies is determined, if the individual faces the gamble of tossing a fair coin, the *ex ante* utility of this prospect is numerically equal to  $\frac{1}{2}$  the *ex ante* utility of the outcome if a head turns up plus  $\frac{1}{2}$  the *ex ante* utility of the outcome if a tail turns up. The full implication of the words "numerically equal" in the previous sentence must not be overlooked. The axiom system implies that if the individual through introspection and serious meditation contemplates the value of his utility function for the fair-coin gamble, he will arrive at a number which identically equals the expected value calculation. Therefore the Bernoulli theorem is no more nor less than a computing rule determining the value of various lotteries given the values of the component parts. The theorem does not state that the individual focuses any attention on the conceptual meaning of the "expected value of utility."

Letting each entry  $U_{ij}$  in the normal form game matrix for a particular player be the *ex ante* value of his utility for the corresponding pair of strategies, if the player randomizes his strategies, picking each with probability  $p_i$ , then for each of his opponent's strategies  $j$ , the player enjoys the *ex ante* utility  $\sum_i p_i U_{ij}$ . If his opponent randomizes his strategies with probabilities  $q_j$ ,

then his *ex ante* utility for the game is  $\sum_i \sum_j p_i U_{ij} q_j$ . Furthermore, given the opponent's randomized strategy  $q_j$ , the player's own Bayes strategy is defined as picking an action which maximizes  $\sum_i U_{ij} q_j$ . It follows that given the opponent's strategy, the player always has at least one pure strategy  $i$  which is optimal. Under such an assumption, the game problem collapses to a problem of simple utility maximization.

An important corollary of the theorem that our utility function is additive is that any positive linear transformation of an individual's Bernoulli utility function  $aU + b$ ,  $a > 0$ , is permissible; in other words, the Bernoulli utility function is determined uniquely only up to origin and scale.

### III. Utility and Money

Some of the remarkable confusions regarding game theory stem from the identifying of utility with money transfers, and Luce and Raiffa throughout are careful to maintain the distinction. If we find it to our liking to assume that money and utility are related by a linear function,<sup>29</sup> we of course are

<sup>29</sup> I.e.,  $aU + b = \text{monetary payoff}$ .

at liberty to do so. But the stimulating articles of Friedman and Savage, and Markowitz<sup>20</sup> have led many economists to argue against such an assumption, except perhaps in expository presentations for beginners, or for a game with insignificant payoffs. It seems to us that much of an economist's potential interest in game theory might very well stem from the fact that the players do not have linear utility functions for money.<sup>21</sup> Although in particular instances, some games using linear approximations for the utility functions in the relevant range of monetary outcomes may be permissible (in any case, such an assumption should be made explicit), in general the identifying of money and utility contradicts the theorem that the utility function is bounded<sup>22</sup> (i.e., Axiom 3).

It occurs to us that the argument against a linear relationship between money and utility provides additional insight for considering only a single play of the game. Imagine a lottery where a coin is tossed for nonnegligible money stakes for both players. At one level of income an individual's utility function may indicate that he should play the game, i.e., his *ex ante* utility for the game exceeds his utility for not playing the game. After a single play of the game, the individual is at another point on his income-utility function. Although the money outcomes and probabilities remain the same for the game, the utilities of the final outcomes may have changed sufficiently to cause the game to be a poor gamble. In short, the player moves from an income at which he is prone to gambling to an income at which he is prone to insurance. Therefore, what is a good strategy for the first play of the game may not remain desirable if the game is played a second time, somewhere beyond the current planning horizon of the individual.

We may draw a second important conclusion from the distinction of money and utility. We have stated that the individual's behavior is unchanged if we make any positive linear transformation of his Bernoulli utility function. If money and utility were linearly related, then an individual who finds a fair bet desirable at \$1 stakes would also find attractive a fair bet at \$1000 stakes, at a \$1,000,000 stake, etc. We reject the hypothesis that such is generally true, and therefore, as Luce and Raiffa warn, we should be on guard against arguments aimed at game theory which are based on a confusion between utility of money and money itself.

#### IV. Two-Person Games

Slightly altering Luce and Raiffa's sequence of presentation, we begin our discussion of two-person games by citing a fundamental fact about all two-

<sup>20</sup> M. Friedman and L. J. Savage, "The Utility Analysis of Choices Involving Risk," *Jour. Pol. Econ.*, Aug. 1948, LVI, 279-304; H. Markowitz, "The Utility of Wealth," *Jour. Pol. Econ.*, May 1952, LX, 151-58.

<sup>21</sup> If both players' utility for money functions are linear, then there is a linear relationship between the two utility functions.

<sup>22</sup> The well-known St. Petersburg Paradox can be derived if monetary payoffs, which are assumed unbounded, are erroneously averaged rather than the bounded utilities of such payoffs. The St. Petersburg lottery is the chance of winning  $2^n$  dollars (or more accurately, rubles) with probability  $2^{-n}$ ,  $n = 1, 2, \dots, \infty$ ; the expected monetary payoff is infinite, although the gamble is not too attractive (even in dollars).



person (and indeed  $n$ -person) games in normal form. Nash proved that there is at least one pair of strategies such that, if each player uses his strategy in this pair, the other player is not induced to change his own strategy in maximizing the expected value of his utility. Such a pair of strategies is said to define an equilibrium point.<sup>33</sup> An equilibrium strategy may entail the use of probability mixtures of the pure strategies, and assures each player of at least the utility he can expect from his minimax strategy (which also may involve the use of probability mixtures).

What are the ramifications of Nash's theorem? Just as an equilibrium set of prices in a Walrasian system need not be unique, there may be more than one equilibrium point in a game; furthermore, the expected utilities of the outcomes for the individual players vary with different equilibrium points. For some game situations (e.g., duopoly) it may turn out that there is a pair of strategies which does not define an equilibrium point but which assures each player a value of *ex ante* utility greater than that for any of the equilibrium pairs. Finally, if each player chooses his minimax strategy, the resulting pair of strategies need not define an equilibrium point.

Von Neumann and Morgenstern investigated in some detail a special case of a two-person game, which is most often presented in elementary expositions of the theory. Luce and Raiffa have termed such games as strictly competitive (Ch. 4; Append. 1 through 7). Let  $U_1$  and  $U_2$  be player one's and player two's utility functions, respectively, where the  $U$  functions are defined according to the axioms in Section II. Further, suppose there exists a positive linear transformation  $aU_1 + b = U_1^*$ ,  $a > 0$ , such that  $U_1^* = -U_2$ ; in other words, we postulate that it is possible to transform  $U_1$  into  $U_1^*$  by a linear operation such that  $U_1^* + U_2 = 0$ . Then von Neumann proved in effect that (a) all pairs of minimax strategies are equilibrium points, and (b) every equilibrium point yields the same magnitude of utility to each player (i.e., the same absolute value of utility).<sup>34</sup> Thus, in this special case, minimax and equilibrium strategies are equivalent.

Luce and Raiffa note that one of the most significant results of von Neumann and Morgenstern's study is that probability mixed strategies provide the key to the existence of equilibrium positions in conflict situations (pp. 170-171). In models where the opponents do not utilize mixed strategies, there is no way of guaranteeing that an equilibrium situation always exists. If there is to be a *theory* of games, and if it is inadmissible to assume an asymmetry between players which is ever present to resolve any conflict, then it is essential to establish that some strategies are available for *both* players which do not involve some sort of mutual contradiction.

When each individual's utility function for money is linear, a game in which the sum of the money payoffs is constant (e.g., zero) falls under the assump-

<sup>33</sup> Pp. 106-9. J. F. Nash, "Equilibrium Points in  $n$ -Person Games," *Proc. of Nat. Acad. of Science*, 36, 1950, 48-49; "Non-Cooperative Games," *Annals of Math.*, Sept. 1951, LIV, 286-95.

<sup>34</sup> Von Neumann proved the magnificent theorem that under the above assumptions, not only do (a) and (b) above hold, but at least one equilibrium point exists in every such game; Nash, following von Neumann's keen insight, demonstrated the universal existence of equilibrium strategies for any  $n$ -person game in normal form.



tions of von Neumann and Morgenstern's analysis. But we are not required to assume that the money utility function is linear, and in any case it is the utility of money which is at the heart of the matter. Therefore we see no reason to suppose in general that the two players in a game need be *reluctant* to participate, for as we realize in the usual case of consumer behavior analysis, the utility function gives an ordinal scale and not one which indicates absolute measurements of pleasure and pain.<sup>35</sup>

When game theorists such as Luce and Raiffa attach the term "optimal" strategy to the minimax action in a zero-sum two-person game, they only mean by this that (a) there is no other strategy which can assure the player of doing better regardless of what his opponent does, and (b) if the opponent uses his minimax strategy, then the player can do no better than by using his own minimax strategy. Perhaps such criteria do not warrant the use of the word "optimal"; this is a matter of taste.<sup>36</sup> But in any case, there is no implication that if a player has a priori information as to what his opponent is going to do, he nevertheless should blindly play the "optimal" strategy. Consequently, in our belief the arguments of game theory critics that the minimax theory completely overlooks the chance of an opponent making a mistake are pretty much beside the point. What such arguments usually amount to is that a player has some a priori information or supposition that his opponent may choose a nonminimax strategy; if this is so, there is no question that the player should select his corresponding Bayes strategy. Furthermore, there are occasions when more than one minimax strategy exists, in which case it is possible to select that minimax strategy which will take advantage of mistakes.<sup>37</sup>

It seems to us that another empty refutation of the optimality of a minimax strategy is one which confuses *ex ante* with *ex post* utility. Needless to say, after the game is over, each player may reprehend himself for not having

<sup>35</sup> D. Ellsberg, "Theory of the Reluctant Duelist," *Am. Econ. Rev.*, Dec. 1956, XLVI, 909-23.

<sup>36</sup> Although we have taken great pains to stress that the fundamental concepts of game theory are defined for a single play, a quasidynamic approach to the minimax theorem has been offered by Brown and Robinson in a framework which is related to Samuelson's Correspondence Principle. By postulating certain dynamic behavioral reactions, we are led to each player receiving his minimax value of utility in the "long run." The behavioral assumptions are identical in spirit to those made by Cournot for the duopoly problem. Each player assumes his opponent's strategy will remain the same (*i.e.*, as he has observed in the previous plays of the game), and then he chooses a Bayes strategy accordingly. If the pair of minimax strategies is unique, Robinson has shown that the process of repeated plays will produce long-run strategies converging to the equilibrium point; in any case there is a convergence to the minimax value of utility. The familiar arguments against Cournot behavior can similarly be applied here to shed doubt on the hypothesis that the behavioral assumptions are realistic, and a fortiori that the minimax strategy is ideal for a single play of a game. See pp. 442-46; J. Robinson, "An Iterative Method of Solving a Game," *Annals Math.*, LIV, 1951, 296-301; G. W. Brown, "Iterative Solution of Games by Fictitious Play," *Activity Analysis of Production and Allocation*, T. C. Koopmans, editor, New York 1951, Ch. 24, pp. 374-76. P. A. Samuelson, *Foundations of Economic Analysis*, Cambridge 1947, p. 284.

<sup>37</sup> Pp. 77-81. M. Dresher, *Theory and Applications of Games of Strategy*, RAND Corp., R-216, Dec. 1951, pp. 69-71.

acted differently. If player two uses a minimax strategy which randomizes among several pure strategies, player one *may* do better in the end by selecting a nonminimax strategy. But the ground rules of the game are that he must select his strategy before the play begins and with or without information about his opponent. Consequently, our utility axiomatization of the manner in which he views risky situations is an *ex ante* approach, which has the effect of weighting *every* possibility with its associated probability (subjective and/or objective).

A more subtle point lurks in the criticism that if an opponent plays his minimax strategy in a two-person zero-sum game, the other participant does not always need to play his own minimax strategy to obtain the minimax level of utility. We can find a parallel controversy in debates concerning revealed preference in standard consumer theory.<sup>38</sup> The point simply is whether it makes sense to distinguish "higher order" preferences among alternatives on the same indifference locus.<sup>39</sup> For example, in the game of Figure 2, each opponent's minimax strategy is to play the choices with fifty-fifty probabilities, assuring a utility valuation of 0 regardless of whether or not the other player

		Player Two's Strategies	
		$T_1$	$T_2$
Player One's Strategies	$S_1$	-1	1
	$S_2$	1	-1

FIGURE 2

utilizes minimax. But if player one realizes that player two is going to randomize as stated, he may prefer the implied superlottery associated with his first strategy over that implied for his second strategy, according to a higher order differentiation of the two possibilities; and analogously for player two.<sup>40</sup> But a revision of each player's strategy destroys the equilibrium associated with the minimax selections. Thus, if *in fact* individuals do have a hierarchy of choice mechanisms among alternatives, then optimality criterion (b) above (p. 380) no longer holds (*i.e.*, if the opponent uses his minimax strategy, the player may do better by using something other than minimax on the basis of higher order criteria). Once the equilibrium property of the minimax pair of strategies is lost, the adjective "optimal" would be inappropriately associated

<sup>38</sup> I. M. D. Little, "A Reformulation of the Theory of Consumer's Behavior," *Oxford Econ. Papers*, Jan. 1949, I, 90-99. Also see references in footnote 24.

<sup>39</sup> In mathematical parlance, the question is whether the individual has a "lexicographic" ordering pattern. See R. M. Thrall, *op. cit.*, pp. 181-86, and M. Hausner, "Multidimensional Utilities," in the same volume, pp. 167-80.

<sup>40</sup> Note that we are still assuming the utility functions at *every* gradation of preference admit a zero-sum formulation. In cases where this premise is false, we would need to consider a complicated noncooperative game (defined below).

with the minimax action, for a player may be able to do better if his opponent plays "optimally" and he does not. We note that with few exceptions, economists and game theorists alike have tended to avoid such issues.<sup>41</sup>

Once we leave the realm of strictly competitive games, problems multiply rapidly and every author feels entitled to his own concept of rational behavior. The situation is regrettable, although not surprising, for herein lie most games of interest in economic analysis. Luce and Raiffa (Ch. 5 and 6) follow Nash's subclassification of conflicts into noncooperative games, in which there is no preplay communication between opponents before the selection of strategies, and cooperative games, in which preplay activities are admissible. Two of their examples readily point up the difficulties in noncooperative games.

In Figure 3, both  $(S_1, T_1)$  and  $(S_2, T_2)$  are equilibrium strategies; that is, if either player were informed as to his opponent's choice, he would not be induced to revise his own selection.<sup>42</sup> But player one prefers the first equilibrium pair to the second, and vice versa for player two. The minimax strategies are  $(\frac{2}{5}, \frac{3}{5})$  and  $(\frac{3}{5}, \frac{2}{5})$  for players one and two, respectively, assur-

Player One's Utilities				Player Two's Utilities			
		Player Two's Strategies				Player Two's Strategies	
		$T_1$	$T_2$			$T_1$	$T_2$
Player One's Strategies	$S_1$	3	0	Player One's Strategies	$S_1$	2	0
	$S_2$	0	2		$S_2$	0	3

FIGURE 3

ing each a utility of  $\frac{6}{5}$ . Notice that both players benefit more from any equilibrium pair than from their minimax strategies. Furthermore, the pair of minimax strategies are not in equilibrium; given that player one (two) chooses minimax, player two (one) is better off to select his second (first) strategy.

If a player could issue an ultimatum before the other did, then the indeterminacy as to which equilibrium pair would result might be resolved, provided that such an act does not change the other player's utility function in such a way as to induce him spitefully to choose the "wrong" response.<sup>43</sup> Luce and Raiffa point out (p. 94) that even if rapport exists between the players to the extent that without any physical communication they should agree that a "fair" solution would be to randomize between the two equilibrium points on a fifty-fifty basis, they are nevertheless thwarted in their goal be-

<sup>41</sup> Hausner and Thrall, *op. cit.*, discuss the possibility of a series of higher-order criteria, but in this reviewer's opinion they do not really face up to the basic problem.

<sup>42</sup> P. 90. The probability mixtures  $(\frac{3}{5}, \frac{2}{5})$  and  $(\frac{2}{5}, \frac{3}{5})$  for players one and two, respectively, also yield an equilibrium point.

<sup>43</sup> T. C. Schelling, "An Essay on Bargaining," *Am. Econ. Rev.*, June 1956, XLVI, 281-306. H. M. Wagner, "A Unified Treatment of Bargaining Theory," *So. Econ. Jour.*, Apr. 1957, XXIII, 380-97.

cause there is no randomizing probability mechanism over their own strategies that they can use noncooperatively to yield the desired effect.

Another type of difficulty associated with the equilibrium-pair notion (p. 95) is exhibited in Figure 4. In this case, each player considering his own utility function selects his second strategy, jointly yielding a utility of 1 for each player. These choices in fact comprise the only equilibrium pair. Yet it is mutually advantageous for them *both* to change to their first strategy, if they could be guaranteed against a "double cross."

		Player One's Utilities				Player Two's Utilities	
		Player Two's Strategies				Player Two's Strategies	
		$T_1$	$T_2$			$T_1$	$T_2$
Player One's Strategies	$S_1$	9	0	Player One's Strategies	$S_1$	9	10
	$S_2$	10	1		$S_2$	0	1

FIGURE 4

Cooperative games are built on the assumption that preplay messages are possible, any agreement arising at this stage is binding (*i.e.*, enforceable), and that each player's utility function is both known by the opponent and not altered in the course of negotiations. The first step in analyzing such conflicts is to delineate all possible outcomes including both those which are a result of noncooperative behavior (akin to the development of normal form pure strategies) and those which can be reached by agreements. Part of this characterization hinges on whether there exists a commodity commensurate with utility which is transferable by means of side payments. If transfers are allowable, game theorists seem to agree that the two players should settle on a solution which maximizes the total joint payoff and afterward resolve the thorny problem of dividing the spoils. Otherwise the consensus is that the settlement of the conflict should narrow to the Pareto optimal outcomes, where once again the particular point on the utility frontier is the problem at issue. In both cases the arguments mainly rest on normative considerations.

There is no need for us to repeat here the familiar reservations regarding the limiting of conflict solutions to Pareto optimal points. We also mention that game theorists apparently have overlooked that even if there are no legal prohibitions against side payments, the agreement to use such should be part of the game itself.<sup>44</sup> The noteworthy "advance" that more recent game theory has made over previous economic analysis is to suggest a single point on the Pareto locus which should be called *the* solution.<sup>45</sup> Luce and Raiffa describe in nice detail the theories of Nash, J. C. Harsanyi, L. Shapley, R. B. Braith-

<sup>44</sup> The reviewer is indebted to R. L. Bishop for this criticism.

<sup>45</sup> Of course, one notable exception to this statement is the commonly cited point of intersection of the players' offer curves. Another is F. Zeuthen's analysis in *Problems of Monopoly and Economic Warfare*, London 1930.

waite, and Raiffa (Ch. 6).<sup>46</sup> The various treatments purport either to characterize how people react dynamically in conflict situations, or to suggest a system of fair and reasonable principles by which the conflict should be arbitrated. With a sufficient number of additional axioms or behavioral postulates, the authors are able to find unique solutions to any conflict problems. As might be anticipated, the solutions themselves differ from theorist to theorist, and consequently even if one is wedded to the notion that economists should adopt a theory of conflict resolution, there is still a wide variety of theories from which to select. Luce and Raiffa briefly hint how such a selection might be made (pp. 121-124).

In addition to the ground rules adopted for cooperative games, the crucial aspects of all of the proposed solutions are the assumptions as to interpersonal utility comparisons and egalitarian principles, threat possibilities during negotiations, the range of alternatives to be considered pertinent, and the definition of a "no agreement" situation. As any economist who has tried to fit a model into game-theory terms knows, it is no simple matter to construct a game of anything but the crudest of economic conflicts, and even these present complications if a numerical solution is sought.<sup>47</sup> Nevertheless, it is our view that game theorists have made a notable contribution to the methodology of economics by demonstrating the possibility of resolving situations which seemingly have a multitude of outcomes by means of postulating the desirable characteristics of a solution. Like any axiomatic approach, the fundamental assumptions may always be criticized. But in our opinion game theorists have suggested a number of axiom systems which are sufficiently reasonable to warrant our concluding that the technique is legitimate as well as stimulating.

#### V. *n*-Person Games

In addition to all the difficulties which arise in two-person games, an entirely new realm of issues appears when a third person is added to the conflict. In the two-person cooperative game, the players may either agree or act independently. As soon as another player appears, there are the additional possibilities of each opponent coalescing with the new entrant, as well as a tripartite agreement. As more players are added, the number of potential combinations enlarges substantially. Once the coalition question is settled, there remains the problem of dividing the gains among the members of the team (provided transfers are permissible).

Luce and Raiffa devote one-fourth of their analysis to an up-to-date taxonomic survey of the *n*-person game literature, and these chapters comprise the least satisfactory section of their book (Ch. 7-12). Some of the fault lies in the lack of a persuasive theory for *n*-person games. The rest is due to Luce and Raiffa's departing from a style which they used so successfully in their other chapters. Very little motivation accompanies the various theories, simple examples are scarce, and the criticisms are often obscure. The authors discuss von Neumann and Morgenstern's characteristic function notion,  $\psi$ -stability,

<sup>46</sup> Also see Wagner, "A Unified Treatment," *op. cit.*

<sup>47</sup> J. P. Mayberry, J. F. Nash, and M. Shubik, "A Comparison of Treatments of a Duopoly Situation," *Econometrica*, Jan. 1953, XXI, 141-54.

Milnor's and Shapley's axiomatic approaches, and in addition, a game-theory determination of voting power in legislatures, and the testing of  $n$ -person theory by means of laboratory experiments. In spite of the obvious importance of  $n$ -person games, relatively little research has been done in this area, and applications are few and far between.

A simple three-person example of Luce and Raiffa serves to illustrate the crux of the conceptual problems (pp. 199-201). Suppose that if each of the players, X, Y, and Z, act independently, they will receive a utility of zero; if any two or all three cooperate, the coalition will receive a total utility of one, and any player outside the coalition receives zero utility (assume there is a transferable commodity commensurate with utility). If, say, X and Y tentatively form a coalition in which each receives a utility of  $\frac{1}{2}$ , then player Z can tempt X, say, into a countercoalition by promising him a utility of  $\frac{3}{4}$  (Z would then get  $\frac{1}{4}$  instead of his present zero). But once the new coalition is tentatively formed, Y can tempt Z into an agreement by promising him a utility of  $\frac{1}{2}$ , etc. Even the possibility of a three-way agreement in which each opponent gets a utility of  $\frac{1}{3}$  does not produce a stable outcome. The various proposed "solutions" to such vexing examples as these comprise either a listing of all possible outcomes which are alleged to be reasonable (analogous to the narrowing of solutions to the Pareto locus), or a selection of an outcome which is uniquely determined by a set of axioms.

Two conclusions are readily apparent from Luce and Raiffa's summary. First, we feel that aside from illustrating the extreme difficulty of analyzing  $n$ -person games, the present literature is only of limited value to economists. Second, we conjecture future solutions will probably rely more on the dynamics of conflict situations. It is perhaps notable that fourteen years after the publication of von Neumann and Morgenstern's work, Hurwicz' remark is still pertinent: "The potentialities of von Neumann's and Morgenstern's new approach seem tremendous and may, one hopes, lead to revamping and enriching in realism a good deal of economic theory. But to a large extent they are only potentialities; results are largely a matter of future development."<sup>18</sup>

## VI. Decision Theory

In their final two chapters, (13 and 14) Luce and Raiffa survey the fundamentals of modern statistical decision theory and K. J. Arrow's approach to group decision-making. Their thorough and clearly written presentation of these topics is sufficient reason to justify their treatment in a book mainly devoted to game theory. But in addition there is a significant parallelism in the methodology of game theory and of decision processes.

Modern statistical decision theory and game theory overlap at two points, viz., the statistician is assumed to enjoy a Bernoulli utility function, and many statistical problems are characterized by a normal-form game matrix. But differences between the two subjects are at least as important as the similarities. It is misleading to consider Nature, the second player in most statistical

<sup>18</sup> L. Hurwicz, "The Theory of Economic Behavior," *Am. Econ. Rev.*, Dec. 1945, XXXV, 909-25.



games, as a willful opponent of the statistician, and certainly the zero-sum formulation is meaningless. Furthermore, it is possible for the statistician to "spy" on Nature's strategy selection by taking sample observations. In a manner clearly reminiscent of the axiomatic solutions to two-person nonzero-sum games, various authors, including A. Wald, Savage, H. Chernoff, Hurwicz, Milnor, J. L. Hodges and E. L. Lehmann, have explored sets of postulates which lead to a unique decision rule for the statistician (pp. 278-309). The uncomfortable conclusion reached by Chernoff and Milnor is that there is no rule which will satisfy all of a certain set of presumably reasonable criteria. The resolution of this paradox is obvious, although disheartening; at least one of the allegedly reasonable properties must be foregone.

Luce and Raiffa make clear the fundamental controversy raging between the modern and neoclassical approaches to statistical inference (pp. 318-24). Decision theorists argue that the crux of any problem of inference is the final action to be taken, and this action carries with it a level of utility (or loss) depending on the true state of Nature. Once the statistician's utility function over final outcomes and a decision rule for uncertain situations are specified, the problem is essentially solved. Such an approach is sufficiently general to incorporate most of the neoclassical results as special cases. The counter-argument to this approach is that in effect the generality is only a diversion; that in most real situations where the tools of statistical inference are used, it is not very helpful to speak of a statistician's Bernoulli-type function indicating a numerical loss for different mistakes. We believe that as with game theory proper, the future ascendancy of statistical decision theory will depend largely on whether the approach suggests more meaningful ways to solve problems of empirical inference.

The connection between game theory and Arrow's *Social Choice and Individual Values*<sup>49</sup> is that group choice (in Arrow's context) might be viewed as an  $n$ -person game where the final outcome is selected by some aggregative scheme, such as voting. Arrow also uses the technique of suggesting a set of presumably reasonable properties which a method of aggregation should satisfy, and then demonstrates that there is no such scheme which obeys all the postulates. In addition to summarizing Arrow's arguments, Luce and Raiffa also survey the principal contributions which have followed Arrow's book, including those by L. A. Goodman and Markowitz, C. Hildreth, D. Black, C. H. Coombs, and May (pp. 345-57).

#### VII. *The Future of Game Theory*

Although Luce and Raiffa do not devote a particular section to the likely new developments and future influence of game theory on the social sciences, certain indications seem clear. The Bernoulli utility function will continue to provide the mainstay for the analysis of decision problems in stochastic situations. Although the minimax strategy may not be offered as "the optimal" rule for the selection of a strategy in the face of uncertainty, it will remain a procedure worthy of serious consideration in such circumstances. Develop-

<sup>49</sup> New York 1951.



ments in  $n$ -person theory are likely to be of a dynamic and behavioristic nature, perhaps embodying Marschak's notion of a "theory of teams."<sup>50</sup> Consequently, there will be a tendency to drop the normal-form abstraction of a game (append. 8). Of some interest to economists will be forthcoming research on games of economic ruin and market equilibrium.<sup>51</sup>

In our opinion, Luce and Raiffa (if not most game theorists) have up to now committed an error of judgment by overlooking the important insights from models of competitive economic behavior which have evolved over the last 120 years.<sup>52</sup> Luce and Raiffa have chosen to remain silent about any contributions falling outside the confines of mathematicians' analyses published after 1944. Nevertheless, the economist will find their book an excellent means of learning of the current status of game theory.

<sup>50</sup> J. Marschak, "Elements for a Theory of Teams," *Manag. Sci.*, 1955, I, 127-37. "Report of the Third Conference on Games," P. Wolfe editor, Princeton University, March 1957. O. Helmer, "The Prospects of a Unified Theory of Organizations," *Manag. Sci.*, 1958, IV, 172-76.

<sup>51</sup> Pp. 483-484. M. Shubik, *Competition, Oligopoly, and the Theory of Games*, forthcoming. L. Shapley, "A Symmetric Market Game," RAND Corp., RM-1533, 1955.

<sup>52</sup> Luce and Raiffa even describe Zeuthen's theory of warfare by means of a secondhand report, pp. 135-37.

## THE DOLLAR SHORTAGE RE-REVISITED

### *A Review Article<sup>1</sup>*

By C. P. KINDLEBERGER\*

The year 1957 will go down in the history of literature on dollar shortage as a vintage one. The crop of books on the subject is a bumper one, including the four volumes under review and at least two others bearing on the subject, which are reviewed separately in this journal.<sup>2</sup>

The present books are diverse. Their authorship is preponderantly British (3:1), but in form they include a book of lectures, a monograph, a textbook and a thesis. More important, they represent a variety of analytical points of view and recommendations for remedy. For better or worse, there is no long study representative of the viewpoint of Hazlitt's *Will Dollars Save the World?*<sup>3</sup> or the preconversion Harrod of *Are These Hardships Necessary?*<sup>4</sup> For this position, if not for the polemics, one must go to the first chapter of Triffin or to Mikesell's testimony before the Boggs Committee with its quotable statement: "Dollar shortage should be regarded as a myth; it is only a cloak used to cover up the fallacies of governmental policymakers."<sup>5</sup> But if the spectrum is not so wide as to include this viewpoint, it is broad enough.

### *I. Analysis of the Shortage*

MacDougall's monograph, which has been awaited for years, is a major work. Its subtitle, "A Study of International Economics," emphasizes that it is addressed to wider issues than persistent disequilibrium. Its structure is complex, difficult to follow in a straight reading: a text of 17 chapters, with footnotes on text; 47 appendices, with footnotes on the appendices. These

\* The author is professor of economics at Massachusetts Institute of Technology.

<sup>1</sup> G. Crowther, *Balances and Imbalances of Payments*. (Boston: Grad. School Bus. Admin., Harvard University. 1957. Pp. vii, 70. \$2.00.)

D. MacDougall, *The World Dollar Problem*. (New York: St. Martin's Press. London: Macmillan. 1957. Pp. xvii, 622. \$12.50; 50 s.)

W. M. Scammell, *International Monetary Policy*. (New York: St. Martin's Press. London: Macmillan. 1957. Pp. xiv, 402. \$9.00.)

E. Zupnick, *Britain's Postwar Dollar Problem*. (New York: Columbia University Press. 1957. Pp. xv, 256. \$5.50.)

<sup>2</sup> S. E. Harris, *International and Interregional Economics*, New York 1957, reviewed on page 485, this journal. R. Triffin, *Europe and the Money Muddle*, New Haven 1957, reviewed this journal, March 1958, XLVIII, 188.

<sup>3</sup> New York 1947.

<sup>4</sup> London 1947.

<sup>5</sup> Subcommittee on Foreign Trade Policy of the House Committee on Ways and Means, *Foreign Trade Policy*, Washington 1957, p. 460.

last, to be sure, are minimal, but a reader, finding himself in an appendix footnote, having left the text for a text footnote where he is referred to an appendix, may be forgiven for pausing a moment to remember whence he started. But even if it does not recommend itself to him who runs as he reads, it remains a major contribution: analytical, empirical, judicious and restrained—rich in double negatives. It summarizes much of the earlier literature and fits it into place; it reviews statistical findings not only on balances of payments, elasticities, income elasticities and output, but also on productivity. Students will be mining its rich veins of fact and idea for years to come.

Discussing it with me, a friend objected that the chapter on remedies for the dollar problem was independent of the preceding analysis. But that is almost always the case. My own misgivings turn on the nature of the analysis.

MacDougall starts out with essentially the "absorption" approach. He examines the United States for possible deflationary tendencies, the rest of the world for signs of inflation-mindedness, and compares productivity in the two halves of the world. Having found no basis for a dollar problem in these relations, he proceeds to analyze what he calls the "structural changes," saying:

Any of these [monetary and productivity] tendencies could cause a deficit but, provided they were not too pronounced, they might be offset by other changes, which we shall call "structural," favorable to the rest of the world's balance of payments. Equally, even if all the dangers mentioned were avoided, unfavorable "structural" changes could still cause imbalance (p. 145).

He then projects ranges of the possible variation in the significant items in the United States balance of payments for 1975, reaching a deficit of \$4 billion on optimistic assumptions, but a surplus of \$16 billion on pessimistic.

This partial-equilibrium technique of analysis is unsatisfactory. J. E. Meade was reprimanded by Ragnar Nurkse<sup>6</sup> for using domestic policies to get full employment, and flexible exchange rates to correct the balance of payments, without allowing for effects of changes in the balance of payments back on income and savings. Whether it is a national trait or not, MacDougall's approach is similar to Meade's. How could the United States have an *ex ante* surplus of \$16 billion from structural causes without savings outrunning investment in the United States and investment in excess of savings abroad? MacDougall is of course too sharp an economist to remain consistently on this partial-equilibrium level. On pages 346-47 he points out that a tendency to surplus in any other country, such as Germany or Switzerland, is of lesser consequence because of their smaller size: these countries would have to save impossibly large percentages of national income above domestic investment expenditure to embarrass the world. At page 343 he notes that, if the rest of the world happened to have a balance-of-payments surplus with the United States, "it is quite likely that . . . the countries would pursue bolder policies which offset any such favorable tendency." In discussing a rise in the price

<sup>6</sup>R. Nurkse, "The Relation between Home Investment and External Balance in the Light of British Experience, 1945-1955," *Rev. of Econ. Stat.*, May 1956, XXXVIII, 142-43.

of gold, he states (p. 402) that there would be "no guarantee that the increase in output would not be sold to the United States . . . so that the next crisis would merely be postponed."

MacDougall recognizes a number of asymmetries. Lowering prices abroad improves the rest of the world's balance of payments less than raising them worsens it (p. 333). This is partly a function of the assumed readiness of the United States to protect imports (which is not, by the way, examined with the scepticism applied to the world's easy generalizations about productivity differentials); and partly a consequence of this country's more plentiful supply of capital and technology which makes it easier for the United States to replace imports than for foreign countries (p. 334). Supply elasticities in the United States are high for foodstuffs. But in estimating supply elasticities in manufactures—very broadly, to be sure, and with proper humility because of the weakness of the data—MacDougall guesses that the elasticities in the United States and the rest of the world are each 5 (p. 572). I should have thought that another asymmetry existed here, as in import-competing industry, and that it was fundamentally related to the dollar problem: a higher supply elasticity with respect to price for United States exports than for those of the rest of the world. This has certainly been the case in the late 1930's, during the war and the immediate postwar period. It may not be specifically true of 1958, but it still seems to be a better generalization than that the supply elasticities are equal.

United States exports of manufactures are seen to rise from \$7.7 billion average in 1953-55 to \$17.7 billion on optimistic assumptions (for the rest of the world) and to \$21.7 billion on pessimistic assumptions. This expansion is largely the result of the structural fact of the United States' ability to supply the "latest thing." In this context (pp. 202, 204) creeps back the "demonstration effect" which had been disposed of under the discussion of "living beyond one's means" (pp. 60-64). Moreover, the demand for these products abroad is inelastic with respect to price in the short run (pp. 211ff), and unlikely to be affected by devaluation.

The reviewer is disposed to ask whether a disequilibrium arising from the production of new goods by the United States and their purchase by, say, Europe is structural or monetary. Suppose the United States produces and Europe buys a new good. Equilibrium can be restored by an expansion of income in the United States which is expended on other exports or spills over into imports; or a contraction of income in Europe which releases resources for the production of other exports or imports-competing goods. But if, to limit ourselves to Europe, resources do not respond to declines in income by shifting into other occupations, or if the monetary authorities react to a loss of domestic employment by an expansionary policy, what is the nature of the disequilibrium? Is the trouble in the disturbance or in the failure of the adjustment mechanism? MacDougall's analysis, and a fortiori that of Scammell and Zupnick, are to blame structural changes—the elasticities rather than the absorption approach.

We can treat the Scammell and Zupnick analyses more quickly. Scammell's

book is not mainly about the dollar problem but a spirited and well-written defense of the Keynes plan against the White plan. He is interested in expanding international reserves through a central-banking mechanism for central banks. The nature of disequilibrium is less interesting to him than that of the adjustment mechanism—the gold standard, flexible exchange rates, and the Bretton Woods and regional systems which have grown up. The exposition of these institutions is beautifully done in clear, straightforward prose with an occasional neat phrase (mostly jibes at the United States?—"carefully prepared and served to suit the delicate digestion of Congress," p. 139; "further pressure . . . served only to thicken the fog of abstract nouns surrounding the American explanation," p. 143). But his analysis of the dollar disequilibrium is elusive. There were three main reasons why prior to 1914 there was no sterling shortage: dependence on overseas commodities, large-scale investment overseas, and a structure of income-inelastic imports (which held up in depression) and income-elastic exports (which declined) (pp. 312-13). These conditions have not been duplicated by the United States. Beyond this, Scammell furnishes short-run explanations for particular United States surpluses: recession and restrictive import policy in the 1930's (p. 318), favorable terms of trade postwar (p. 322), and a gold price policy which "robbed the non-dollar world of one means of dealing with the American current account surplus" (p. 323), etc. He then returns to broad "structural" explanations—the constant rise in American productivity which means that "whether full employment is maintained in the United States or not, there is little hope of Europe competing effectively with the United States" (p. 339); the "tides of world demand" which for "at least twenty years . . . have been flowing towards the United States" (p. 346). One of his most interesting suggestions, tossed off in a footnote, but worth extended analysis, is that a significant difference between the United States and Britain during the periods of their economic supremacy was that the latter was being overtaken, as the United States is not, by faster rates of growth of productivity abroad (p. 356).

Scammell's main theme is the necessity for enlarging international reserves by amending the International Monetary Fund, which he attacks as a "mere mouthpiece by American economic power" (p. 394). (His book, it is important to say, was written before the 1956 revival of activity of the Fund in connection with the Suez monetary crisis, and he would possibly want to modify some of his remarks about its inactivity in the light of the record which he failed to anticipate.) He recognizes, moreover, that this action might not enable the scarcity of the dollar to be cured, and would supplement it with other lines of action. But we recur to questions of policy.

Zupnick's thesis suffers many of the faults of its genus. It summarizes material which has been summarized before; it is poorly written, with much jargon, a surfeit of adjectives and adverbs, and some solecism. It is studded with exasperating quasiquantitative remarks, such as:

In the light of the recent empirical studies which show that American price elasticity of demand for British exports in the interwar period was extremely high, it is reasonable to assume that a sufficiently large tariff reduction, applied in a nonselective manner, would ultimately result in a

rather substantial increase in the volume of American imports derived from the United Kingdom (pp. 116-117).

and

... the extremely large capital outflow imposed a rather heavy burden on the United Kingdom (p. 151).

But the main difficulty is that Zupnick indulges, with MacDougall and Scammell, in partial-equilibrium reasoning. He gives pride of place in causing the British postwar dollar problem to the rise in the United States price level and the dominant position of the United States oil companies. There are six reasons in all; the other four include the inadequacy of primary production abroad; the increased tempo of industrialization abroad; the failure of the gold price to rise; and the failure of the United Kingdom to adapt to the basic changes which occurred in the structure of the world economy. But the first two stand out for him. "It is difficult to exaggerate the importance of this conclusion" (p. 120).

It is easy to exaggerate the importance of the rise in the United States prices and the dominant position of the United States oil companies as causes of the British dollar problem. One could as readily argue that American prices were too low if they let the British buy all those imports they could not pay for; or, more reasonably, that the European demand caused the rise in prices rather than the increase in prices the large expenditure. But even if the rise in prices in the immediate postwar period had been an autonomous variable caused by supply conditions, the essential problem is why Britain did not adjust to it, and whether, if prices had been lower, all other items in the British balance of payments would have been unchanged, to produce a *pro tanto* improvement in the balance. Zupnick has misgivings on this score when he discusses the price of gold. On two occasions (pp. 130, 220ff), he recognizes that a higher price for newly produced gold would mean more exports, but it also might mean more spending. In the final analysis, improvement in the British balance of payments requires an increase in income larger than the associated increase in spending, and this would not be guaranteed by any change in prices or in ownership of oil companies.

Geoffrey Crowther's book has all the virtues of a set of lectures by an Englishman to an American audience. It is polished and witty. Moreover, the analysis rests at basis, although at times it seems to get away from it, on the fundamental fact that balance-of-payments surpluses and deficits are the mirror image of deficits and surpluses of domestic spending over income.

The first lecture sets out the relation of the evolution of the balance of payments to various stages of national growth, paralleling Shakespeare's seven stages of man and running in terms of the difference between spending and income. This analysis follows that apparently developed earlier in Crowther's *Outline of Money*, first published in 1941. The third lecture on dollar scarcity, however, tends to depart from absorption and to deal in terms of "natural resources and the structure of different national economies" (p. 45):

It is difficult to believe that there can ever have been another case of a country where the demand of the rest of the world for its products was

so urgent, and its demand for the products of the rest of the world so indifferent—where rises in price would choke off so few sales on the one side and falls in price stimulate so few purchases on the other—as is the case with the United States today (p. 48).

In industry after industry, it is not simply that the American machine is better or cheaper, it is the only one obtainable that will do the job (p. 46). There are so many American goods that the world wants, whatever they cost (p. 51).

But even if structure produces United States import demands of zero price elasticity for price decreases and zero income elasticity for income increases, and rest-of-world import demands for United States goods of zero price elasticity for price increase and zero income elasticity for income decreases—which MacDougall's careful investigation shows to be far from the case—it is still true that a surplus on the part of the United States means absorption less than income, and a deficit in the rest of the world absorption in excess of income. Why does the job, which only the American machine can do, have to be done? If foreign countries buy United States goods whatever they cost, the explanation must be tied back into the impact on the propensities to spend as a whole, and not merely on imports.

## II. Policy Recommendations

Zupnick is not particularly concerned with therapy. The three Britons are. MacDougall is opposed to flexible exchange rates, in contrast with Scammell, who opts for them. Along with Scammell, however, he favors raising the dollar price of gold and additionally enlarging international reserves. But his main reliance is on the maintenance of the machinery of controls and the readiness to discriminate against United States goods if need be. Devaluation may be inevitable, and presumably the appropriate medicine for deep-seated trends against Europe. But it would be a mistake to dispense with the equipment needed for exchange control designed to discriminate against the dollar. Until reserves are built up to absolutely safe amounts, it may be necessary to apply it while trying to decide whether an adverse movement is secular or merely a fluctuation about a neutral trend.

Crowther's solution is two (non-Communist) worlds. He thinks it will be possible to limit ourselves to two, since the adjustment mechanisms of income and flexible exchange rates which will not work between the Eastern Hemisphere and the dollar area, function effectively within Europe and between Europe and the sterling area (for different reasons in the two cases, to be sure). Two worlds, in his judgment, will be tolerable: the United States has come a long way since Undersecretary of State William L. Clayton's and Ambassador Lewis Douglas' insistence on freer trade to its present support of the Common Market, the Free Trade Area and the Coal and Steel Community.

Scammell, finally, is not very clear whether his main policy recommendation—the enlargement of international reserves—will in fact enable the dollar problem to be cured (p. 357). Like Thorp in *Trade, Aid or What?* (Balti-



more 1954) he is for Trade, Aid and Everything. Recognizing that it is easier to shoot down five (*sic*) separate assaults than a mass attack, he would go after dollar shortage in Europe with a program of expanding exports to the United States, earning dollars in multilateral exchange, reducing imports from the United States, adoption of flexible exchange rates, higher United States overseas investment, and a higher dollar price of gold. But his main interest, as indicated, is to enlarge the volume of international reserves (apart from the increased gold price). He passes over the basic difficulty of the International Monetary Fund which has been not its small size but its incapacity to deal with trends, as opposed to fluctuations about trends. Scammell's remedy is the same as Triffin's; but, while he recognizes the existence of a dollar shortage, and the necessity to deal with it by other means, Triffin in his enthusiasm for multilateral clearing is under no obligation to deal with a trend because he does not believe it to exist.<sup>7</sup>

Serious objection may be raised against a point accepted by MacDougall (pp. 263ff) and Scammell (p. 351) relating to capital movements as a means for offsetting excesses and deficiencies of absorption. Following an analysis developed by Walter Gardner, Randall Hinshaw, Walter Salant and Evsey Domar, they worry lest foreign lending add to the dollar problem rather than solve it by requiring a flow of dollars from the borrowing countries to the United States for interest and amortization. But this reasoning is in error since it neglects increases in productivity in the borrowing country and increases in income in the lending. Crowther is on stronger ground in admitting that the theoretical case for lending is a sound one, but that the practice bristles with difficulties (pp. 54ff).

Crowther also has the better of the argument, in my judgment, on the relevance of tariff policy to the disequilibrium. Scammell wants tariff reductions as part of his broad attack on disequilibrium (pp. 340-41); MacDougall asserts that the United States could "help most by a progressive liberalization of commercial policy" (p. 379). A useful illustration of the superiority of the Crowther position of irrelevance is furnished by the lack of impact of German tariff reductions on her balance.

### III. *Concluding Remarks*

How far have we come? Certainly a long way on the details: on the measurement of the elasticities, on productivity in general (though not on the introduction of new goods), and on the general theory of balance-of-payments disequilibrium. But there is still much to be done to unify the theories of income and price in international economics, to bridge the gaps in Crowther's analysis leading from absorption to structure, and to amend the partial-equilibrium approach used by MacDougall for the purpose.

I am still disposed to argue against Mikesell that there is no asymmetry in the distribution of wisdom among governmental policymakers (at least

<sup>7</sup> Having mentioned Seymour Harris' book earlier, it may be appropriate to place it among the others by stating that its analysis is eclectic and classificatory, and that it eschews recommendations for coping with the dollar shortage (which it understands to exist).

in so far as the United States is concerned, on the one hand, and the rest of the world ex-Germany and ex-Switzerland, on the other). I imagine, too, that he, Haberler, Furth, Triffin—to bring in more names mentioned in Harris' eclectic summary—would be prepared to bet with MacDougall and Crowther that there was greater likelihood that a given future disequilibrium in the balance of payments of the United States would involve dollar shortage than dollar surfeit. At the same time, however, it behooves MacDougall and Crowther to make a more general case for persistent disequilibrium than new goods, urgent demands, United States quickness to impose tariffs, natural resources, or a more mystical "economic strength." None of these considerations apply to Germany and Switzerland, except possibly tariff in the latter country. And in none can the connection between structure and the balance of payments be made unless one goes through and explains the relationship between spending and income.

I am persuaded that the disequilibrium is systematic. But interesting, provocative, and full of insight as the 1957 crop of books is, there is still room, in my view, for further work.

# THE PERMANENT INCOME HYPOTHESIS<sup>1</sup>

## *A Review Article*

By H. S. HOUTHAKKER\*

The consumption function, once as controversial as any Keynesian innovation, is now almost universally accepted as a tool of economic analysis. It would be difficult to find a recent cycle or growth model of which it is not a cornerstone. Uncontroversial though the *notion* of a consumption function has become, about its mathematical *form* there is much less agreement. Yet the study of macroeconomic models has shown that the exact shape of the consumption function has a considerable influence on the stability properties of the model.

Two questions about the consumption function are of particular importance. The first is whether consumption is proportional to income, so that the average and marginal propensities to consume coincide; Keynes himself had suggested that the average propensity would normally exceed the marginal propensity. The second question is whether income should be regarded as past, current or expected, or as some combination of the three.

Much empirical research has been devoted to these questions, especially to the first one. At first family budget surveys were the chief source of information, and the consumption functions derived from them seemed to bear out Keynes' postulate of disproportionality. The short time series available until 1940 led to much the same conclusion. Then, however, Simon Kuznets' estimates of savings going back to the end of the 19th century became available and they presented quite a different picture, which was subsequently confirmed by R. W. Goldsmith's more elaborate calculations. If Keynes had been right the ratio of consumption to income should be higher 50 years ago than it is now. Yet the long-term estimates revealed no significant change in that ratio. This clear contradiction between the evidence from time series and from cross-section data cannot be resolved by casting doubt on the statistics; it calls for a more comprehensive theory which accounts for each of the two conclusions.

The proposed solutions to this contradiction have all taken the form of a reinterpretation of the independent variable in the consumption function. Closest to the original Keynesian formulation is Tobin's approach, which

<sup>1</sup> A review of *A Theory of the Consumption Function* by Milton Friedman. National Bureau of Economic Research General Series 63. (Princeton: Princeton University Press. 1957. Pp. 243. \$4.75.)

\* The reviewer is professor of economics at Stanford University, currently on leave at Massachusetts Institute of Technology. He is greatly indebted to Malcolm M. Jones and the M.I.T. Computation Center for carrying out the calculations reported in this review.

introduces assets as additional explanatory variables.<sup>2</sup> A more radical departure, which took "income" to mean the relative income position of a household in its community, had already been defended by Dorothy S. Brady and Rose Friedman, J. S. Duesenberry and Franco Modigliani.<sup>3</sup> Of immediate relevance to the book under review was the point of view taken by Hamburger who, in an unpublished doctoral thesis (Chicago 1952) emphasized the relations between consumption and wealth.<sup>4</sup>

There is some empirical support for each of those approaches, but it can hardly be said that they have been definitely established. There is still room for another hypothesis, and the purpose of this most stimulating and closely reasoned book is to present one. Although the application to consumption is new, the principal ideas go back to some earlier work in which the author participated.<sup>5</sup>

### I. The Underlying Theory

Milton Friedman's hypothesis is based on a division of both consumption and income into a permanent and a transitory component. By permanent income is meant the annual equivalent of the revenues which a person expects to receive during a long period of time (presumably several years, though not necessarily his entire life). Later in the book (p. 93) we are told that permanent income "is to be interpreted as the mean income regarded as permanent by the consumer unit in question, which in turn depends on its horizon and foresightedness." It will accordingly depend on his accumulated or inherited capital, occupation, environment, etc. The transitory component consists of unforeseen additions or subtractions to income, which are supposed to cancel out over the period considered and to be uncorrelated with permanent income.

Friedman's fundamental relation is between permanent consumption and permanent income; in fact the *ratio* between permanent consumption and permanent income is supposed to be dependent only on the rate of interest, the ratio of "nonhuman" wealth to income, and individuals' tastes and to be independent of permanent income. The sum of the permanent and the transitory component, which is the only magnitude we can observe from cross-section data for a single year, is called "measured" income (or measured consumption as the case may be). Moreover (and this is the crux of the hypothesis) the transitory component of consumption is assumed to be uncorrelated with the transitory component of income; measured consumption depends consequently on permanent income (with an error term), but not on measured income as such.

What this amounts to in less technical language is that the household plans

<sup>2</sup> See James Tobin, "Relative Income, Absolute Income, and Savings" in *Money, Trade and Economic Growth, Essays in Honor of John Henry Williams*, New York 1951, p. 135 ff.

<sup>3</sup> See particularly J. S. Duesenberry, *Income, Saving and the Theory of Consumer Behavior*, Cambridge Mass. 1949.

<sup>4</sup> See also William Hamburger "The Relation of Consumption to Wealth and the Wage Rate," *Econometrica*, Jan. 1955, XXIII, 1 ff.

<sup>5</sup> See Milton Friedman and Simon Kuznets, *Income From Independent Professional Practice*, New York 1945.

consumption over a fairly long period, on the basis of expected income during that period and that it will not diverge from its consumption plans because income in a particular year falls short of or exceeds expectations. The marginal propensity to consume out of transitory income would therefore be zero. The man who has a lucky day at the races does not buy his friends a drink, and the poor fellow whose wallet is stolen does not postpone the purchase of a new overcoat. In other words, the availability of liquid assets has no influence on consumption decisions.

This startling conclusion is derived in Chapter 2, principally from an analysis of the case of complete certainty. Friedman apparently does not share the doubts frequently expressed concerning the relevance, and indeed the logical consistency, of the assumption of certainty. Even so, some rather strange arguments have to be invoked before the permanent income hypothesis can be deduced. Thus on page 13 it is argued that a dollar spent in year one must be equivalent to a dollar spent in year two because "the things being compared are of the same stuff, differing only in dating," and the author finds it "hard to see any reason why this difference in dating should have an asymmetrical effect."

Other authors have not found it so hard to see why time introduces asymmetry. On page 167 Duesenberry is chided for maintaining the classical view that the higher an individual's income, the more attention he can afford to pay to future needs. Friedman comments that: "as is shown in Chapter 2, this analysis is, to say the least, most unsatisfactory on a purely theoretical level." In fact, Chapter 2 shows nothing of the kind; it simply evades the issue. The theoretical level, incidentally, is so pure that consumers are assumed to be not merely omniscient but immortal (p. 12, fn. 8). Friedman's argument implies also that the rate of interest on loans is zero, for the sum borrowed and the sum repaid are "of the same stuff, different only in dating."

The case of complete certainty is so remote from reality that the author's views on the matter might be thought unimportant. Unfortunately, this is not so, for Friedman's brief discussion of uncertainty amounts to little more than an assertion that uncertainty makes no difference. The discussion is unsatisfactory chiefly because it pays little or no attention to liquidity constraints. It is a matter of common observation that people are often unable to borrow in order to improve the time-shape of their consumption, even though their prospects might justify borrowing. This imperfection of the capital market is perhaps the principal reason why consumption can frequently not be adjusted to permanent income. Later in the book (p. 93, fn. 51) Friedman himself mentions this imperfection as one reason why permanent income might vary with age, but he ignores the point in his theoretical chapter.

The theoretical foundations of Friedman's approach, it will be seen, are anything but firm. Let us not forget, however, that in his well-known methodological essay<sup>6</sup> Friedman has argued that a theory should be judged

<sup>6</sup>See his *Essays in Positive Economics*, Chicago 1953, Part I.

not by its assumptions, but by its conclusions. However this may be, we must now look at his empirical evidence.

## II. *The Statistical Analyses*

There are no data on permanent income, and to test the hypothesis it is necessary to translate it into observable terms. More particularly, the postulated relation between permanent income and permanent consumption has to be translated into a relation between measured income and measured consumption. This is done in Chapter 3, where it is shown that the elasticity of measured consumption with respect to measured income is equal to the fraction of the variance of measured income contributed by the permanent component of income. Thus if permanent income could be measured directly (in which case its variance would equal the variance of total income) the elasticity just mentioned would be equal to 1, hence consumption would be proportional to income. The greater the contribution of transitory components, the smaller the elasticity.

Although empirical analyses occupy most of the book, I will not devote much space to them in this review. Friedman's argument is mostly so intricate and subtle that a brief discussion could not do justice to it; but I will have more to say about the empirical evidence in Part III. It would be unjust, however, to omit an expression of admiration for the skill and insight which he displays throughout those chapters. Much of what he has to say is debatable, but all of it is thought-provoking and intelligent. In his ability to relate observations to hypotheses Friedman is without peer.

Chapter 4, perhaps the best in the book, presents the evidence in favor of the permanent-income hypothesis that can be derived from cross-section data. Chapter 5 performs a similar task with reference to time-series data. Together these two chapters make out a good *prima facie* case for the permanent-income hypothesis. On the whole that hypothesis does what it is intended to do: it agrees both with the disproportionality between measured income observed in the cross-section data, and with the proportionality observed in the time series data. In Chapter 6 the permanent-income hypothesis is compared to the relative-income hypothesis; it is shown that the two lead to broadly similar conclusions. Despite Friedman's evident sympathy for the relativists, however, he rejects the sociological contentions that are basic to their approach.

I feel the more justified in refraining from detailed criticism of this empirical evidence because it has only an indirect bearing on the validity of the permanent-income hypothesis. The data are shown to be consistent with that hypothesis, but since they are also consistent with a number of other hypotheses, they do not confirm Friedman's hypothesis. To put it differently, the relation asserted between permanent income and permanent consumption is not identified in the data he considers.

In this respect Chapter 7, which is devoted to an estimation of the marginal propensity to consume from income data only, is of greater interest. As mentioned before, the elasticity of measured consumption with respect to measured

income is equal to the proportion of the variance in total income attributable to the permanent component. The latter proportion can be estimated if income data for a number of years are available for the same spending units. Such data are relatively spotty, but for what they are worth they indicate that the proportion for nonfarm families is between .7 and .85. Since regression analyses of consumption on income lead to rather similar coefficients Friedman regards this coincidence as strong evidence for the permanent-income hypothesis. Actually the possible range of the proportion is so wide, and the households to which the two sets of evidence refer are so different, that it is hard to be impressed. In the one case where the two analyses are made on the same set of data (Table 20) the correspondence is anything but close, and it is useless to attribute this to deficiencies in the data.

### III. *Additional Testing*

A more searching test is therefore needed. Such a test has in fact been proposed, but not performed, by Friedman himself (pp. 215-16). Actually he does not consider this test, or the others suggested in the same Chapter 10, to be necessary for confirmation of the permanent-income hypothesis, for in his opinion the evidence he has examined already provides sufficient confirmation. It will be clear by now that I do not share this view, and I am all the more grateful to Friedman for indicating a further testable implication of his hypothesis. Interestingly enough, his confidence in the favorable outcome of this test was premature, as we shall see.

The test now to be discussed is based on cross-section data, in which the participating households are classified into subgroups so as to reduce the variability of permanent income. To take a simple example, the households are classified according to the occupation of the head, and within each occupation group, according to income. It is then possible to calculate a regression of measured consumption on measured income for each of the occupations separately, and also for all occupations combined. According to Friedman the marginal propensity to consume out of transitory income is zero, and hence the elasticity of measured consumption with respect to measured income is equal to the proportion of the variance of total income that is accounted for by permanent income. Thus if we compute a regression for steelworkers only, the elasticity should be less than for the population as a whole, because there is less variation in permanent income amongst steelworkers than there is amongst people from all occupations combined. Since therefore the variability of measured income observed in the case of steelworkers must be due more to transitory components, the elasticity should be smaller than in the case of all occupations taken together. Extending this reasoning to multiple classifications Friedman points out that his hypothesis implies that the elasticity should be smaller the more narrowly defined the group of households for which it is computed.

The large-scale budget survey carried out by the U. S. Bureau of Labor Statistics in 1950 and published jointly with the Wharton School of Finance



TABLE I.—MARGINAL PROPENSITIES TO CONSUME (UPPER FIGURE) AND INCOME ELASTICITIES (LOWER FIGURE) BY CITY CLASS, OCCUPATION AND AGE OF HEAD, U.S. URBAN CONSUMERS, 1950

Occupation	City Class									All	Age of Head			
	North			South			West				25-34	35-44	45-54	55-64
	Large	Suburban	Small	Large	Suburban	Small	Large	Suburban	Small					
Self-employed	.438 .527	.648 .692	.562 .463	.512 .565	.535 .576	.822 .537	.582 .464	.424 .386	.437 .469	.505 .523	.391 .304	.595 .588	.421 .436	.549 .645
Salaried professionals, officials, etc.	.591 .744	.863 .852	.622 .757	.743 .797	.507 .643	.795 .836	.623 .741	.587 .727	.678 .812	.650 .770	.703 .756	.631 .733	.742 .796	.580 .800
Clerical and sales workers	.724 .754	.818 .813	.812 .922	.773 .779	.875 .749	.430 .439	.790 .743	.701 .249	.418 .489	.758 .726	.808 .836	.714 .698	.778 .665	.732 .723
Skilled wage earners	.768 .740	.613 .598	.639 .692	.919 .875	.661 .758	.787 .663	.710 .572	.891 .877	.909 .895	.762 .740	.780 .788	.794 .714	.747 .809	.746 .646
Semiskilled wage earners	.834 .833	.796 .704	.983 .888	.808 .840	.942 .928	.612 .783	.876 .796	.760 .755	.754 .752	.827 .815	.713 .753	.883 .868	.822 .822	.895 .792
Unskilled wage earners	.841 .721	.828 .846	1.083 1.049	.890 .911	1.020 .903	.869 .890	.817 .805	.914 .591	.943 .796	.874 .816	.914 .960	.870 .814	.854 .787	.869 .784
Not gainfully employed	.834 .571	.686 .546	.905 .457	.827 .659	.738 .845	1.059 .964	.639 .541	.816 .465	.630 .288	.773 .530	.981 .651	.713 .765	.767 .503	.761 .503
All	.596 .679	.732 .738	.667 .628	.759 .792	.637 .775	.824 .801	.696 .682	.600 .553	.557 .557	.658 .696	.713 .740	.669 .744	.617 .669	.667 .656
AGE														
25-34	.695 .720	.634 .675	.576 .689	.762 .859	.817 .869	.644 .830	.797 .615	.878 .683	.570 .717	.713 .740				
35-44	.647 .791	.725 .811	.780 .841	.792 .817	.607 .748	.941 .575	.689 .640	.481 .591	.470 .599	.669 .744				
45-54	.506 .585	.847 .816	.693 .723	.690 .728	.568 .762	.896 .885	.745 .689	.629 .493	.651 .539	.617 .669				
55-64	.715 .677	.634 .648	.540 .484	.847 .797	.698 .706	.695 .856	.606 .699	.780 .469	.544 .494	.667 .656				

and Commerce<sup>7</sup> provides great scope for testing this implication, thanks to the many cross-tabulations presented. The particular classification chosen was by city class, occupation, and age of the head of the household. The groups contained under each of those headings can be seen in Table I. In order to avoid meaningless results based on very small numbers of households, all those where the head was over 65 or under 25 were excluded, and so were all remaining subcells in which fewer than 5 income groups were represented.<sup>8</sup> This left 215 subcells containing 9923 households. Moreover these subcells were combined in various ways, for instance by lumping all cells in the same occupation.

For each of the 363 cells so created two simple regressions of consumption on income were computed. One was linear (constant marginal propensity to consume) and the other double-logarithmic (constant elasticity of consumption with respect to income). Friedman uses both shapes of the consumption function, though on the whole (and I agree) he prefers the latter.<sup>9</sup>

Reproduction of the full results of those analyses would occupy too much space. The marginal propensities and the elasticities for the larger cells (those where the households were classified by two characteristics, by one characteristic, or not at all) can be found in Table I. This table contains three cross-classifications, one by occupation and city class, one by occupation and age of the head, and one by age of the head and city class. The entries in the row and column marked "All" are common to pairs of cross-classifications, which made it efficient to combine everything in one table.

A quick glance at Table I shows that Friedman's assertion, according to which the regression coefficients for more narrowly defined subgroups should be smaller than for broader subgroups, is by no means borne out. Looking, for instance, down the "All" column for various occupations, we see that only one occupation (the self-employed) has an MPC smaller than the .658 found for the sample as a whole. A similar check for the elasticities shows that only two occupations have a smaller elasticity than the sample as a whole has. Similarly, of the 36 MPC's and elasticities in the cross-classification by age and city class, only 15 are smaller than the corresponding MPC's and elasticities in their city-class group (that is to say, smaller than the item in the "All" row at the top of this cross-classification).

A more systematic comparison of this type is summarized in Table II. The "minor" cells are those which are defined by more characteristics than the "major" cells. Separate comparisons are made for linear and double-logarithmic regressions. Cases are called "favorable" if they are in accordance with Friedman's assertion, that is if the estimate for the minor subgroup is smaller than for the major subgroup; otherwise they are called "unfavorable." The comparisons made in the previous paragraph for the cross-classification by age

<sup>7</sup> *Study of Consumer Expenditures, Incomes and Savings*, Vol. II, Philadelphia: University of Pennsylvania, 1956.

<sup>8</sup> By "subcell" is meant a group of households belonging to the same city-class group, occupation group and age group.

<sup>9</sup> Within each cell the averages for each income range were weighted by the number of households in that range. The informed reader will realize that access to the IBM 704 was a distinct advantage.

TABLE II.—COMPARISON OF REGRESSION COEFFICIENTS  
IN CELLS OF DIFFERENT CONTENT

Defining Characteristics		No. of Cells		No. of Cases			
				Linear		Double-log	
Minor Cells	Major Cells	Minor	Major	Favorable	Unfavorable	Favorable	Unfavorable
1. Age of head	All households	4	1	1	3	2	2
2. Occupation	All households	7	1	1	6	2	5
3. City class	All households	9	1	4	5	5	4
4. Age & occupation	Age	28	4	8	21	12	16
5. Age & occupation	Occupation	28	7	16	12	16	12
6. Age & city class	Age	36	4	15	21	17	19
7. Age & city class	City class	36	9	16	20	16	20
8. Occupation & city class	Occupation	63	7	29	34	28	35
9. Occupation & city class	City class	63	9	24	39	28	35
10. Age, occupation & city class	Age and occupation	215	28	102	113	94	121
11. Age, occupation & city class	Age & city class	215	36	82	133	96	119
12. Age, occupation & city class	Occupation & city class	215*	63	101	110	98	113
13. Age, occupation & city class	All households	215	1	67	148	86	129

\* Four major cells contained only one minor cell each, so that no comparison was possible.

and city class, for instance, can be found in line 7 under "linear" and "double-log."

Table II shows that for almost every comparison the number of unfavorable cases exceeds that of the favorable cases. Even though most of the comparisons are not statistically significant taken separately, the combined impression is very damaging to the permanent-income hypothesis. Perhaps the most devastating evidence is in line 13: of the marginal propensities to consume in the 215 smallest subcells less than one-third agree with Friedman's theory, and only 40 per cent of the corresponding elasticities agree. Even if the coefficients for the minor cells had a fifty-fifty chance of being smaller than the coefficients for all households, the observed result would have a probability of less than one per cent. A fortiori we are compelled to reject the hypothesis that the coefficients in the minor subcells are on the whole smaller than those for all households.

#### IV. Evaluation

The negative conclusion, which applies equally well to the linear and to the double-logarithmic form of the consumption function, clearly reflects adversely on the permanent-income hypothesis of which it is the most direct

test available so far.<sup>10</sup> The fact that Friedman himself had confidently suggested it only adds to its significance.<sup>11</sup> Nor can there be any doubt that if the concept of permanent income has any meaning at all, its numerical value must be more uniform in the minor than in the major subcells, so that the conditions for the test are indeed satisfied.

It is more difficult to say, however, what the negative result of the test actually implies. One obvious though tentative interpretation is that the MPC out of transitory income, far from being zero as Friedman maintains, is actually greater than the MPC out of permanent income. Thus, to come back to our previous example, the lucky winner does not run to the savings bank but to the tavern, and the victim of theft does cut his coat according to his cloth. It is perhaps relevant here that the definition of consumption used in the test is not the one that Friedman prefers. He would like to exclude the purchase price of durables from current consumption and include their use value. I have much sympathy for this idea, which was first put into practice by Goldsmith in his savings study referred to above. Unfortunately durable purchases are not stated separately in the tabulations of the BLS Survey of 1950, nor in many of the sources which Friedman uses in his book. In any case, I doubt whether this conceptual difference can account for the negative result of the test.

The main thing that needs to be done, it would seem, is to make explicit allowance for the factor that is so conspicuously absent in Friedman's theory, namely liquid assets. Here again, the BLS-Wharton data are deficient. If it could in fact be shown that liquid assets do not influence current consumption, the permanent income hypothesis may yet become a useful tool of economic analysis. As of now it must be regarded as a novel idea whose interest lies neither in its theoretical plausibility nor in its empirical validity, in both of which respects it is unsatisfactory, but in the further research which it is likely to stimulate.

<sup>10</sup> I understand that Harold Watts of Yale University has independently performed a similar test using data from the Survey of Consumer Finances and has reached similar conclusions.

<sup>11</sup> I should point out, however, that I have diverged from Friedman's suggestion in one minor respect. Friedman's conclusion is that a weighted average of the coefficients in the minor cells should be less than the coefficients for the major cells. He does not say, however, what weights should be used. Any plausible set of weights would no doubt lead to the same negative conclusion as was reached above. Thus if total income is used as a weight, the mean of the MPCs in the 215 smallest sub-cells is .734, as against .658 for all households combined.

## COMMUNICATIONS

### The Mutual Influence of Mitchell and Commons

Professor Boulding in his address, "A New Look at Institutionalism," at the 1956 annual meeting of the American Economic Association, observed: "Of the three [best representatives of the school], Commons is the isolate—and to my mind probably the most important and influential of the three in the long run. It is curious that neither Veblen nor Mitchell seem to have known Commons, though it is hard to believe that they did not have at least a casual contact."<sup>1</sup>

It is true that Veblen and Commons seem to have had little or no personal contact. But the case of Mitchell and Commons is far different. They not only knew each other but corresponded and functioned together professionally, in a variety of significant ways, for some twenty-five years. The evidence is both published and unpublished. The published material includes the following: Mitchell and Commons were in a small group that met in 1917 to promote what became the National Bureau of Economic Research.<sup>2</sup> On its formal organization in 1920, Mitchell became director of research and Commons a member of the board of directors. While Commons was president of the Stable Money League and its successor organization, The National Monetary Association, Mitchell was a member of its Research Council.<sup>3</sup> On becoming president of the American Economic Association in 1924, Mitchell appointed Commons to membership on the Nominating Committee. Finally, in the preface of *Legal Foundations of Capitalism* (1924), Commons lists Mitchell's name as first of those from whom he had "important assistance and criticism."

Then there exists an extensive correspondence between the two men which is in the Mitchell Papers, available in the division of Special Collections of the Columbia University Library. It is by no means complete; if Commons had not destroyed his own correspondence files, it is probable that more letters would be available. But the letters we do have cover the last two decades of Commons' life, a period in which Commons published his major contributions to economic theory.

The Commons-Mitchell correspondence includes discussions and criticisms of theory as well as matters of public policy, preparation of manuscripts and publication strategy, and other professional matters. Mitchell read, criticized, and made suggestions on some of Commons' most important theoretical works, and Commons acknowledged not only the assistance, but the intellectual stimulation of his colleague. They met to discuss research projects and articles.

Here are some interesting passages from the correspondence: It begins with a letter from Commons on December 20, 1921:

<sup>1</sup> *Am. Econ. Rev. Proc.*, May 1957, XLVII, 6.

<sup>2</sup> Herbert Heaton, *A Scholar in Action: Edwin F. Gay*, Cambridge 1952, p. 94.

<sup>3</sup> Irving Fisher, *Stable Money*, New York 1934, pp. 204-5.

Dear Mr. Mitchell,

Received copy of the [National Bureau] volume on *Income [in the United States]*. I am greatly pleased with it. Shall see you at [the American Economic Association meetings at] Pittsburgh. I hope you can let me have a copy of your paper to be read at that time, as I have a seminar this year on Business Cycles and Unemployment.<sup>4</sup> Also I want to talk over with you about sources of materials. . . .

After the meetings, Commons wrote on January 4, 1922:

Dear Mr. Mitchell,

Referring to our conversation on theory of value in Pittsburgh, I am sending herewith an article on Correlation of Law and Economics which I have in course of preparation for a law magazine. I am also sending the first chapter of my latest revision of valuation theories . . . I hope to talk matters over again when I see you in February in New York.<sup>5</sup> . . .

On October 30, 1922, Commons sent to Mitchell the completed manuscript entitled, *Reasonable Value*:

My dear Mitchell,

I am taking the liberty of sending you by express the manuscript of my latest revision of my chapters on value theories based on the court decisions. . . . I shall greatly appreciate whatever comments and criticisms of the chapters you may have. . . .

Mitchell replied on March 29, 1923:

Dear Commons:

. . . Your work will have a profound influence on economic theory and perhaps on jurisprudence, but . . . the development of this influence is likely to be rather slow. . . . The book is too original to have a rapid success. It is also too large a volume to be read by many people. Have you considered the possibility of publishing chapters 1-9 first, giving people a little time to digest that volume, and then coming out with chapters 10-12, perhaps under an independent title? . . .

Commons answered (April 9, 1923):

Dear Mitchell:

. . . I am particularly thankful for your suggestion of issuing it [*Reasonable Value*] as two volumes, making a division as you suggest. I am starting in at once to get the first volume ready and hope to have it completed by next fall. That will give me two or three years to work on the review of the different cycles of economic theory. . . .<sup>6</sup>

The next letter (August 2, 1923), reveals that Commons was so impressed

<sup>4</sup>The Mitchell paper is "The Crisis of 1920 and the Problem of Controlling Business Cycles," *Am. Econ. Rev.*, suppl. March 1922, XII, 20-32.

<sup>5</sup>The reference to New York is apparently to the annual meeting of the board of directors of the National Bureau of Economic Research.

<sup>6</sup>The first part became *Legal Foundations of Capitalism*, New York 1924, and the remainder, *Institutional Economics*, New York 1934.

with Mitchell that he sought, though unsuccessfully, to have Mitchell prepare a brief on "Pittsburgh Plus":

Dear Mitchell:

I am writing you in order to find out whether you could . . . serve the four states of Illinois, Iowa, Minnesota and Wisconsin in presenting the economic facts and arguments before the Federal Trade Commission in the so-called "Pittsburgh Plus" case. . . . These states want to have an outside economist, not a resident of these four states, and have delegated me and my colleagues [from the other state universities to make the choice] and we have agreed on you. . . .

In sending Mitchell chapters of his new theoretical treatise, *Investigational Economics*, Commons wrote on March 30, 1937:

Dear Wesley:

Heartily appreciate your book [*The Backward Art of Spending Money and Other Essays*]. . . . It is just what I was hoping to get—your Presidential address on *Quantitative* etc., bearing on comparison I am making of Lionel Robbins and my *Institutional*, analysis. . . . I am rapping him on his criticism of you & wanted to know just how you would have disposed of him. . . .

Mitchell replied on May 7, 1937:

Dear John:

. . . The manuscripts you sent me . . . succeed admirably in showing the importance of the issues that your analysis raises. . . . I am winding up my year's course on Types of Economic Theory by discussing with the class your *Institutional Economics*. I think a large proportion of them at least feel more drawn toward your kind of work than toward that of any of the other masters whom they have studied. . . .

In answer, Commons wrote, on May 29, 1937:

Dear Wesley:

Enclosed is concluding chapters of my *Investigational Economics* for beginners, along with *Contents* showing the setting. I wonder if my references to you are the kind of reply you would make to Robbins. At my age I keep thinking I may be daffy without knowing it, and reiterating the obvious, or hitting a straw man. Your comments on preceding chapters reassure me. Please comment on this at your convenience. . . .

Mitchell's strong support of Commons' theoretical work both in reviews and correspondence came at a time when dominant opinion as expressed in the great majority of reviews, showed, to put it mildly, little appreciation of its importance.<sup>7</sup>

This correspondence, and the evidence of the published works of these

<sup>7</sup>It should be noted that the economists' skepticism of Commons as a theorist did not diminish their admiration of him as the leading student of American labor. This latter reputation was doubtless responsible for his election to the presidency of the American Economic Association in 1917.



men, suggests that there were many points of agreement and similarity between Commons and Mitchell and that these transcend their differences. But a detailed discussion of this larger point would take us beyond the limits of a note.

JOSEPH DORFMAN\*

\* The author is professor of economics, Faculty of Political Science, Columbia University.

### The Union and Wages in Basic Steel: A Comment

*Editor's Note:* Footnotes 2a, 3a, 7a, 8a and 20a were added by the author after he had read Rees' *Reply*; but Ulman wishes it to be clear that agreement with other aspects of the *Reply* is not necessarily implied.

The purpose of this paper is to challenge the interesting conclusion by Albert Rees<sup>1</sup> that the steelworkers' union did not raise wages appreciably above the levels which would have been reached in its absence during the war-and-postwar inflationary period. This conclusion is based primarily on inferences drawn from: (1) a comparison between wage changes in basic steel and wage changes in certain less well-organized occupations; (2) a comparison between wage changes in basic steel in the two world-war inflationary periods; and (3) reports of shortages in both the product and the labor markets of basic steel in the inflationary period following the second world war.

I will consider each of these alleged foundations in turn and will point out (a) that in each case the criterion is not a satisfactory one for determining whether or not the steelworkers' union made an independent contribution to the observed increases in the earnings of its members in basic steel, and (b) that in certain instances the empirical evidence presented by Rees, when supplemented by additional relevant evidence, makes the contrary conclusion at least as plausible as the conclusion that it did not do so. In the latter connection, it should be noted that Rees at one point merely claimed that "it is equally easy to argue that collective bargaining in the basic steel industry during this period kept wages below the level which they would otherwise have reached, or to argue that it raised wages above such a level." However, it is elsewhere made abundantly clear that he regarded the first inference as more equal than the second. This paper might therefore be regarded as an effort to redress this disparity.

#### I. The Interindustry Wage Comparisons

From a comparison between percentage increases in average hourly earnings in basic steel and in selected industries characterized by much less complete unionization than prevailed in steel, Rees concludes that, for the periods 1939-1948 and 1945-1948, "the efforts of the United Steelworkers were not enough to offset the forces working toward relatively larger increases

<sup>1</sup> Albert Rees, "Postwar Wage Determination in the Basic Steel Industry," *Am. Econ. Rev.*, June 1951, XLI, 389-404.

for other groups" (p. 401). This much is true for the majority of the industries in question, but of course it does not imply that, in the absence of a well-organized union, wages in basic steel would have risen as much as they actually did. The latter inference is implied in the conclusion quoted above only if one could have confidence that, for the purpose at hand, the only significant difference between basic steel and the comparison industries consists in their respective degrees of unionization. But no attempt was made to control for other differences between basic steel and the other industries to which it was compared—such as differential changes in employment and productivity and differences in market structure or in original level of earnings. Other writers believed such differences to be significantly associated with variations in relative wages, and some of them attempted to make allowance for them in assessing the impact of unionism per se.<sup>2</sup>

For example, apart from the difference in the degree of unionization, basic steel differed from most of Rees' comparison industries with respect to certain characteristics which tended to hold wages in basic steel down relative to wages in the comparison group in the early wartime subperiod, and with respect to other characteristics which tended to exert the same effect in the subperiod between the suspension of controls and the onset of recession in late 1948.

In the first place, the original level of earnings in basic steel greatly exceeded that in all other categories except crude petroleum. (See Table II below.) Since, in wartime the labor markets in essential industries were characterized by widespread substitution of less skilled and lower-paid labor for more highly skilled and higher-paid workers, one would expect greater percentage, as well as absolute, increases in earnings in industries which originally employed relatively high proportions of low-paid workers and which sought subsequently to retain them.<sup>2a</sup> This point is related to another element of difference between basic steel and most of the highly organized industries with which it was compared. Over the decade 1940-1950 (which includes the period under discussion), women comprised a high and in some cases an increasing proportion of the work force in certain occupational groupings which are relevant to 7 of Rees' 12 comparison industries: textile-mill products, in

<sup>2</sup> See especially J. T. Dunlop, "Productivity and the Wage Structure," in *Income, Employment, and Public Policy*, New York 1948, pp. 341-62; A. M. Ross, *Trade Union Wage Policy*, Berkeley 1948, pp. 113-33; A. M. Ross and William Goldner, "Forces Affecting the Interindustry Wage Structure"; and J. W. Garbarino, "A Theory of Interindustry Wage Structure Variation," both in *Quart. Jour. Econ.*, May 1950, LXIV, 254-305.

<sup>2a</sup> Another methodological issue is related to the failure of Rees' paper to control for differences in original levels of earnings. Levinson's study does compare groups of highly organized industries with groups of lightly organized industries in the same classes of original earnings; and the author finds that the former did not secure greater wage increases than the latter during the period 1941-47. But a comparison among large groups at once raises the possibility that wage movements in the nonunion sector might be affected by wage movements in the unionized sector. Levinson calls attention to the fact that part of the wage increases in the union sector might have been due to "union pressure" and that "this greater increase might then have been transmitted to the nonunion workers through sympathetic pressure." H. M. Levinson, *Unionism, Wage Trends, and Income Distribution, 1914-1947*, Michigan 1951, pp. 67 and 74.

which 53.3 per cent of all employees were women in 1950 as against 49.9 per cent in 1940; confectionery and related products—65.4 and 67.1 per cent, respectively; laundry and dry-cleaning—67.1 and 66.8 per cent; and wholesale and retail trade—37.9 and 34.1 per cent. In all of these categories, the number of women employed increased despite the fact that, during the wartime period, employment of women in higher-paid defense industries expanded greatly. At the same time, the proportion of women workers to the entire female population 14 years of age and over increased from 26 per cent in 1940 to a peak of 37 per cent in 1945 and then declined to a level of 29 per cent in 1950.<sup>3</sup> But whether or not increased wages were required to attract these additional female workers to the labor force, it is reasonable to presume that these lower-paid trades were under especially strong market pressure to raise wages in order to retain and increase their own female work forces.<sup>3a</sup>

A further element of difference between basic steel and most of the comparison industries consists in the fact that steel was far more "essential" to the wartime economy; and it is a matter of common knowledge that wartime controls on wages were much more effectively imposed on such essential industries than they were on more purely "civilian" trades—such as confectionery, laundries, cleaning and dyeing, retail trade, ice cream, etc.

Moreover, in the subperiod following the removal of wage controls, basic steel's pricing policy tended to retard the increase in its demand for labor. Rees pointed this out and attributed this phenomenon in part to the existence of widespread collective bargaining in basic steel. In Part III below, however, we question this explanation and suggest that the existence of another set of characteristics, its oligopolistic structure and political sensitivity, sufficed to account for this industry's price behavior—in which respects basic steel differed notably from all of the other comparison industries with the exception of crude petroleum.

In addition, Rees' method carries the implication that in none of the industries which were lightly organized did unionism exert an impact upon the level of money wages. This method makes no allowance for the possibility that nonunion firms might have raised wages in order to prevent themselves from becoming organized.<sup>4</sup>

<sup>3</sup> U. S. Dept. of Labor, Women's Bureau, *Changes in Women's Occupations, 1940-1950*, Bull. 253, Washington 1954, pp. 90-97; and *1956 Handbook on Women Workers*, Bull. 261, Washington 1957, p. 2.

<sup>3a</sup> Nor can the movements in wages of male wage earners be regarded as independent of the special influences which affected the wages of their female co-workers. If employers were under especially strong market pressure to raise the wages of women in those industries which had heavy concentrations of female labor, considerations of tradition, equity, and morale alone, in addition to interchangeability in some occupations, would have induced them to raise the wages of their male employees to a certain extent. (Men and women employees in such industries are frequently in the same family!) Slichter notes some ("not pronounced") tendency for the hourly earnings of male common labor to be low where the percentage of women among wage earners is high. He attributes this in part to the fact that employers in those industries might wish to hire inferior male workers, some of whom could not transfer to "heavier" industries where wages are higher. S. H. Slichter, "Notes on the Structure of Wages," *Rev. Econ. Stat.*, Feb. 1950, XXXII, 85.

<sup>4</sup> S. H. Slichter, "Do the Wage Fixing Arrangements in the American Labor Market Have an Inflationary Bias?" *Am. Econ. Rev.*, May 1954, XLIV, 338.

Thus Rees' procedure can support a conclusion no broader than the statement quoted in the introduction—but this conclusion is not directly responsive to the question which he raises at the outset: "Has large-scale bargaining during the postwar period raised wages above the levels which would have prevailed in its absence?" It is clearly possible that, even if—contrary to fact—steel wages had risen more rapidly, in percentage terms, than nonsteel wages during the periods studied by Rees, they might have risen even more rapidly in the absence of widespread collective bargaining. And it is clearly possible that, in the absence of widespread collective bargaining, steel wages might have lagged more, in percentage terms, relative to the other wages than they did in fact.

## II. Comparisons Between Two War Periods

Rees presents data which reveal "that steelworkers made larger percentage gains in money and real hourly earnings during the period 1914 to 1920 than during the period 1939 to 1948. In the earlier period steelworkers' money earnings increased relative to earnings in all manufacturing, while in the more recent period they decreased" (p. 400).<sup>5</sup> However, during the postwar period 1945-1948 average hourly earnings in steel rose as rapidly as average hourly earnings in all manufacturing, and money hourly earnings in both just about kept pace with the cost of living. (The annual increases were 33, 32, and 34 per cent respectively.)<sup>6</sup>

Now it is evident that interpretation of all these findings must be subject to the same class of restrictions which apply in the case of the interindustry comparisons discussed in the preceding section, since no control was explicitly established for all relevant differences between the two periods apart from the difference in the degree of unionization in steel. Comparing basic steel wages relative to wages in all manufacturing in the two periods might be construed as an attempt to control for other differences; however, it also eliminates the differences with respect to unionization. For, unionization in manufacturing increased along with unionization in steel during the second period; by 1946, 69 per cent of the production workers in manufacturing were covered by collective bargaining.<sup>7</sup> Manufacturing wages rose less rapidly in 1939-1948 than in 1914-1920 (see footnote 14), but this does not warrant the conclusion that unionism in all manufacturing had no effect upon the movement of hourly earnings in manufacturing and upon the cost of living during the second period. Hence, the fact that steel wages fell relative to the former and failed to rise relative to the latter by as much as in 1914-1920

<sup>5</sup> The statement concerning the movements in money earnings in basic steel relative to those in manufacturing is supported by computations from data of the National Industrial Conference Board for 1914-1920 and from data of the Bureau of Labor Statistics for 1939-1948. Computations from BLS data for the period 1914-1920 indicate that steel wages rose no more rapidly than did manufacturing wages in the early period. See footnote 14 below.

<sup>6</sup> Average money hourly earnings in basic steel rose from \$1.188 in 1945 to \$1.580 in 1948; in all manufacturing they rose from \$1.023 to \$1.350. See BLS series published in *Mo. Lab. Rev.*, Table C-1. The cost-of-living movement is calculated on both the "old" BLS Index of Consumers' Prices and the revised index.

<sup>7</sup> *Mo. Lab. Rev.*, May 1947, LXX, 765.

might merely indicate that the United Steelworkers were unable to keep up with the Joneses over the period 1939-1948 and specifically in the subperiod 1939-1945.

In any event Rees claims only that his evidence "does show . . . that the efforts of the union were not sufficient to offset apparent differences in other factors, probably including the smaller increase in steel prices . . ." (p. 400), and, as we have seen, unionization in manufacturing in the second period. But we might ask whether one might find other evidence to support the more relevant hypothesis that, but for the existence of widespread collective bargaining in basic steel, the percentage increase in hourly earnings which did occur over the period 1939-1948 would have been smaller than it was—despite the fact that it was much smaller than the increase in 1914-1920.

The latter hypothesis, I believe, acquires greater plausibility than does Rees' view to the contrary when the following additional evidence is arrayed against the wartime wage comparisons: (1) a comparison of changes in employment in basic steel in the two wartime periods, taken in conjunction with (2) the changed nature of the jobs and the work force in this industry.

1. *The Two Wartime Increases in Employment.* According to Table I employment in basic steel increased by much more in 1914-20 than it did in 1939-48, not only in percentage terms but absolutely as well, and relative to total civilian employment and labor force. Thus while steel wages rose more rapidly in 1914-20 than in 1939-48, so did employment in basic steel. Moreover, while the per cent increase in average hourly earnings in 1939-48 was about .6 as great as that in 1914-20, the per cent increase in basic steel employment in 1939-48 was only about .45 as great as that in 1914-20.

Suppose, for illustrative purposes, that one wished to estimate the percentage increase in hourly earnings in basic steel for 1939-48 on the assumption that it bore the same relationship to the percentage increase in employment during that period as the percentage increase in hourly earnings in 1914-20 bore to the percentage increase in employment in 1914-20. Then, given the latter ratio and the observed increase in employment in 1939-48, one would have predicted that hourly earnings would have increased about 71, 66, or 62 per cent in 1939-48, depending upon which estimate of employment for 1920 is used. This is well below the calculated increase of 87 per cent. Moreover, we have not taken into account the fact that, while the increase in steel employment was much smaller in the second period than in the first, the increases in total employment and labor force were much smaller in the first period. Thus, in 1914-20, basic steel claimed between 149 and 172 thousand of the 4,315,000 new jobs created in the economy—roughly  $3\frac{1}{2}$ -4 per cent; whereas in 1939-48, it claimed only about 100 thousand of the 13,628,000 new jobs created—under 1 per cent.<sup>7a</sup> In addition, as the table

<sup>7a</sup> The example in the text might be altered to make some allowance for differential movements in the labor force as well as in purely inflationary components of the wage increases by comparing increases in real wage rates (which, according to Rees' data, amounted to 27 per cent in 1914-1920 and 13 per cent in 1939-1948) with increases in steel employment relative to civilian labor force. In any event, the example is intended merely to illustrate the possibility that the observed increase in wages during the second period might well have

TABLE I.—HOURLY EARNINGS, EMPLOYMENT, AND WEEKLY HOURS IN BLAST FURNACES, STEEL WORKS, AND ROLLING MILLS, 1914-1920 AND 1939-1948

(1) Year or Change	(2) Average Hourly Earnings	(3) Estimates of Employment in Basic Steel			(4) Civilian Labor Force (in thousands)	(5) Civilian Employ- ment (in thousands)	(6) Weekly Hours Basic Steel
		A	B	C			
1920	.745	426,700 <sup>a</sup>	436,295 <sup>b</sup>	450,100 <sup>c</sup>	41,750 <sup>a</sup>	40,080 <sup>a</sup>	63.1
1914	.301	278,072	278,072	278,072	38,875 <sup>a</sup>	35,765 <sup>a</sup>	64.9
Absolute change	.444	148,628	158,223	172,028	2,875	4,315	-1.8
Per cent change	147.5	53.5	56.9	61.9	7.4	12.1	-2.8
1948	1.580		488,500 <sup>d</sup>		61,442	59,378	39.5
1939	.845		388,441		55,230	45,750	35.3
Absolute change	.735		100,059		6,212	13,628	4.2
Per cent change	87.0		25.8		11.2	29.8	11.9

<sup>a</sup> The low estimate (A) for 1920 implies, before rounding, an increase in employment of only 2.4 per cent between 1919 and 1920. It is taken from an index of employment compiled by the National Research Project of the Works Progress Administration in *Production, Employment and Productivity in 59 Manufacturing Industries*, Report No. S-1, Pt. 2, Philadelphia, May 1939, pp. 97-99, and forms the basis of most subsequent productivity studies of this industry for the period 1919-29. It also uses month-to-month link relatives, but is based on the year 1929 and extends the BLS index for 1923-36 back to 1919. Presumably the difference between the low and the high estimates for 1920 reflects the fact that the sample of firms reporting year-to-year changes in employment each month for 1919-20 was different from the sample reporting for the year 1920-21.

<sup>b</sup> The middle estimate (B) for 1920 was obtained by applying an estimated increase of 156.9 per cent to the Census figure of 1914. This estimate was taken from an "adjusted employment index" constructed by the BLS in 1926 (*Mo. Lab. Rev.*, July 1926, p. 9) which reflects changes in hours of work as well as in the number of employees. However, since the number of hours worked declined during this period, this estimate understates the increase in the employment of workers. Like our high estimate, this estimate of the increase in worker employment for the period 1919-20 takes 1914 as a base and uses the Census figures for 1919 and the monthly reports on changes in employment compiled by the BLS in deriving the estimate of employment for 1920.

<sup>c</sup> Estimate (C) for 1920 was obtained by applying to monthly Census data on employment in steel works, blast furnaces, and rolling mills percentage changes in employment reported to the BLS by a sample of employers in "iron and steel" for the 9 months, January through September. The monthly changes in employment for October through December were omitted in this calculation because employment in 1920 was depressed due to the A.F. of L.'s organizing strike and the reported year-to-year increases averaged greatly in excess of the increases reported for the other months. Inclusion of all twelve year-to-year changes would yield an average total of 459.5 thousand production workers in basic steel in 1920.

<sup>d</sup> The estimate for 1948 was obtained by multiplying the BLS figure of 536.8 thousand for 1948 by the ratio of the 1947 item in the Census series to the 1947 item in the BLS series. As the following data show, these proportions show some tendency to decline over the period 1947-50; hence applying the 1947 ratio tends to overstate 1948 employment in the census series.

(Continued at bottom of next page)



suggests, the existence of the basic 12-hour day in 1914-20 made it difficult for the industry to absorb part of its increased man-hour requirements by lengthening the work week, whereas it was able to do this in the period 1939-48.

Nevertheless, the foregoing could be quite consistent with Rees' hypothesis that unionism was not an independent influence in raising money wages in 1939-48 if it is assumed that it was considerably more difficult for basic steel to attract and retain workers in the second period than in the first. Note, however, that this assumed change in the steel labor-market situation would have had to have been of sufficient magnitude to offset the relatively larger increase in labor supply over the second period. But is this assumption plausible? I believe not.

## 2. *The Changed Nature of the Job and the Work Force.* Historical evi-

been less than it was in fact were it not for the presence of a strong union. The evidence presented in this section is consistent with the view that, in the second period relative to the first, the industry's increase in demand for labor was smaller and the increase in the supply of labor available to it was greater. In the next section, further evidence (on immigration) is presented in support of the latter proposition; other evidence will be put forth to support the view that industry's labor supply elasticity was greater in the second period.

(Continuation of Table I)

Year	(1) BLS Series	(2) Census Series	Ratio of (2) to (1)
1947	517.6	470.8	.910
1948	536.8	488.5 estimated	
1949	476.7	427.9	.898
1950	535.6	475.9	.889

For a general explanation of differences between the estimates provided by the two agencies see *Handbook of Labor Statistics*, 1950 ed., p. 3. The estimated increase of about 26 per cent in production employment in blast furnaces, steel works, and rolling mills between 1939 and 1948 may be checked against the estimate provided by the American Iron and Steel Institute series on number of wage earners in the iron and steel industry. According to the latter series, iron and steel employment rose from 396 thousand in 1939 to 503 thousand in 1948, an increase of 27 per cent (See *The Economic Almanac*, 1953-1954, New York 1954, p. 383. This series begins in 1935.)

\* Estimate from unemployment data presented by Stanley Lebergott, "Annual Estimates of Unemployment in the United States, 1900-1954", in Nat. Bur. Econ. Research, *The Measurement and Behavior of Unemployment*, Princeton 1957, p. 216. National Industrial Conference Board data on total labor force (based on the gainful-worker concept) and total employment yield increases of about 5 and 10 per cent respectively for the period 1914-20.

Sources: Average hourly earnings: 1920 and 1914, U. S. Dept. Commerce *et al.*, *Historical Statistics of the United States*, Ser. D164-171, p. 69; 1948, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1950 ed., Table C-1, p. 69; 1939, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1947 ed., Table C-1, p. 54. Estimates of employment in basic steel: 1914, U. S. Bur. Census, *Fourteenth Census of the United States*, Vol. X, Washington 1923, p. 310; 1939, U. S. Bur. Census, *Statistical Abstract of the United States*, 1955, Table No. 1040, p. 837. Civilian labor force and civilian employment: 1948 and 1939, U. S. Bur. Census, *Statistical Abstract of the United States*, 1955, Table No. 220, p. 187. Weekly hours, basic steel: 1920 and 1914, U. S. Dept. Commerce *et al.*, *Historical Statistics of the United States*, Ser. D164-171, p. 69; 1948, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1950 ed., Table C-1, p. 69; 1939, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1947 ed., Table C-1, p. 54.



dence suggests that it was more rather than less difficult for basic steel to attract and retain workers in 1914-20 than in 1939-48. Largely because basic steel was the only major industry in the economy which adhered to the 12-hour day for its production employees, it experienced the highest turnover rate of any of the large manufacturing industries in 1909; and it was reported that "... the ablest and best-trained men in the steel works were leaving the steel industry when opportunity offered, to avoid the 12-hour day, the frequent overtime, and the great irregularity of work."<sup>8</sup> And as a result of its unpopular working conditions, basic steel was obliged to recruit a very large and growing proportion of its work force from the nation's supply of inexperienced and docile Slavic immigrants—many of whom, having left their wives abroad, were more willing to sacrifice leisure time for extra income than were native-born workers.<sup>9</sup> However, after the first world war began, the industry's supply of new cheap foreign labor was abruptly shut off—just when the industry had to embark upon a very large expansion of its work force. Steel employers greatly increased their employment of Negroes from the South during the first world war—as they also did during the second—but the large majority of the new workers had to be recruited from the ranks of white adults already in this country.<sup>10</sup> And the latter had already been exhibiting a marked tendency to leave basic steel. That the wartime increases in steel wages which did occur were in part required by the necessity of overcoming the industry's persisting relative disadvantage in hours of work and other conditions of employment is reflected in the following excerpt from an editorial in *The Iron Age* in 1919.

Labor was more contented ten years, 15 years, 20 years ago than it has been in the past few months. The flow of immigrants to whom steel industry rates and steel industry hours of service were attractive has been shut off completely for five years and seems altogether unlikely to be resumed, yet the steel industry has increased its capacity by 40% dur-

\* U.S. Senate Doc. No. 110, 62nd Cong., 1st Sess., *Report on Conditions of Employment in the Iron and Steel Industry*, Washington 1911, Vol. III, p. 384; also pp. 15, 20, 21, 162, 165-166, 175, 380; also Vol. I, pp. xiv-xvi.

<sup>8</sup> Between 1909 and 1914 the work week was reduced, although to what extent the scheduled levels of hours in 1914 reflected the existence of depression in the industry could not be determined, as the 1914 Census of Manufactures noted. The *Reply* calls attention to this reduction and also to the fact that 20 per cent of all workers in basic steel worked 72 hours or over; in all manufacturing—including basic steel—however, only about 3.5 per cent worked that long in the course of a week.

<sup>9</sup> *Ibid.*, I, xvi, xli-xlii.

<sup>10</sup> According to admittedly inaccurate Census data, 5 per cent of the persons attached to "blast furnaces and steel rolling mills" in 1910 were Negro; by 1920, Negro employment had increased to 16 per cent of the total. Applied to the estimates of basic steel employment in Table I for 1914 and 1920 (and arbitrarily assuming 5 per cent Negro employment in 1914), this would roughly indicate an increase of less than 59,000 Negroes in the work force out of a total increment of 148,000-172,000. See U. S. Bur. Census, *Thirteenth Census of the United States*, Washington 1914, Vol. IV, pp. 338-39; and *Fourteenth Census of the United States*, Washington 1923, Vol. IV, pp. 346-47, 15. For the increase in Negro employment in iron and steel as a whole during the second world war period, see Robert C. Weaver, *Negro Labor, A National Problem*, New York 1946, pp. 297-98.

ing that time. Obviously it is necessary to have more attractive conditions than formerly in order to keep the industry continually manned. The demand has been increased, the supply has been diminished.<sup>11</sup>

But almost immediately after U. S. Steel finally inaugurated the basic eight-hour day in 1923, industry sources reported that the labor shortages with which they had been continually plagued had come to an end; and the secretary of the A. F. of L. committee for steel organization wrote that the 8-hour day "has checked the rush for organization that otherwise would have taken place . . . [because] in the steel mills operating on an 8-hour basis employment therein will be found as desirable as anywhere else. . . ."<sup>12</sup> There can be little doubt that he was substantially correct and that the industry entered the second world war better prepared to compete for production labor than it had been at the outset of the first world war.<sup>13</sup> While the industry had eliminated its disadvantage with respect to hours of work, its wage position vis à vis manufacturing industry in general in 1939 was virtually the same as it had been in 1914 and in 1920.<sup>14</sup> More important, whereas the quit rate in basic steel had been extremely high during the earlier period, it was well below the rate in all manufacturing in 1940; and although the quit rate in basic steel rose more rapidly than the quit rate in manufacturing from 1940-48, the former was still below the latter near the end of the second inflationary period. In April 1940, the quit rate in steel was 0.4 per 100 employees and the quit rate in manufacturing was 0.7; in April 1948, the rates were 2.1 and 3.0, respectively.<sup>15</sup>

<sup>11</sup> *The Iron Age*, August 21, 1919, p. 522. An editorial in 1918 (April 11, p. 956) noted that, "The American born . . . do not gravitate to the blast furnace and steel mill. If the mountain will not come to Mahomet, it is perfectly clear what it behooves Mahomet to do. Wages and conditions that have been satisfactory to one class must be made satisfactory to another class if the other class is to be attracted . . ."

<sup>12</sup> *Ibid.*, Oct. 18, 1923, p. 1,030.

<sup>13</sup> Of course, at the beginning of each of the two wartime periods, there was no immediate stringency in the labor markets, since both 1914 and 1939 were years of high unemployment. Lebergott estimates that 8.0 per cent of the civilian labor force was unemployed in 1914 and 9.7 per cent in 1915; while in 1939, 17.2 per cent were unemployed. See Stanley Lebergott, "Annual Estimates of Unemployment in the United States, 1900-1954," in Nat. Bur. Econ. Research, *The Measurement and Behavior of Unemployment*, Princeton 1957, pp. 1-6.

<sup>14</sup> *Ibid.*, p. 215.

*Average Hourly Earnings*

(1) Year	(2) Basic Steel	(3) All Manufacturing	(4) Steel/All Mfg.
1914	.301	.223	1.35
1920	.745	.555	1.34
1939	.845	.633	1.33
1945	1.188	1.023	1.16
1948	1.580	1.350	1.17
1948, September	1.675	1.386	1.21

Source: *Historical Statistics of the United States*, Ser. D, 164-71, p. 69; *Handbook of Labor Statistics*, 1947 ed., Table C-1, p. 54; *Mo. Lab. Rev.*; *Handbook of Lab. Stat.*, 1950 ed., pp. 58-59, 69.

<sup>15</sup> *Mo. Lab. Rev.*, Table B-2.

We believe that the assumption that it was more difficult for basic steel to retain and recruit the marginal worker in the second period than it had been in the first is difficult to maintain in the face of the historical evidence cited above. Indeed, the contrary assumption appears easier to defend, although it is not necessary to do so. Let one merely assume equal difficulty in both periods and then compare the two periods with respect to changes in steel employment—whether in percentage terms or relative to changes in the labor force—as well as changes in wages. Such a comparison might well lead one to wonder why the 1939-48 wage change was not smaller than in fact it was.

Needless to say, the rough calculations in the preceding subsection cannot be used to show by how much the steel union was responsible for the increase in money wages in 1939-48. For one thing, if we accept the hypothesis that unionism was an independent influence on wages, we must also reckon with the possibility that the increase in basic steel employment would have been greater in the absence of the union than it actually was during that period (and that this would have reduced the difference between the predicted and the observed wage increases).

### III. Shortages and Expectations

Rees would presumably agree that in the absence of the union the increase in steel employment would have been greater than it actually was before the inflationary peak had been passed in September 1948, but for a quite different reason; he believes that the union helped to choke back the demand for steel labor and thus to prevent both employment and wages from rising as rapidly as they would have in the absence of the union. And he refers to reports of local labor shortages during the postwar period as evidence that wages would have risen more than they did had it not been for collective bargaining. Let us now test the third leg of this tripod.

Two alleged causes of local labor shortage are considered. On the one hand, "wage increases were undoubtedly delayed by fixed-term collective agreements" (p. 402). However, such fixed-term agreements need not have resulted in shortages if the periodic negotiations had resulted in wage rates sufficiently high to compensate in advance, as it were, for subsequent increases in demand. Since it is noted in another connection that "shortages existed soon after wage increases were negotiated, indicating that negotiated rates at these times were probably below those which would have prevailed in the absence of collective bargaining" (p. 393), one might infer that the existence of fixed-term agreements is not regarded as the most important agent of retardation.

The more important cause of local labor shortage is alleged to have been the union's contribution to the industry's decision to underprice its product, thereby permitting product shortages to develop and also making it unprofitable to raise wages to levels which would have obtained in the absence of price restraint. In part, the union's publicity campaign against price increases helped to hold down steel prices, according to this argument, but more important was the alleged fact that employers feared to raise prices and wages sufficiently to overcome the "temporary" shortages in steel and steel labor

because they believed that "the union could successfully maintain the higher level of wages after the demand for steel had declined." Hence in the absence of the union, it is alleged, wages would have risen to higher levels during this period.

Can we presume that, in the absence of the union, the existence of local labor shortages would have sufficed to induce employers in basic steel to raise wages above the levels actually reached under collective bargaining? Were any alternatives to raising the basic hourly rate available to basic steel employers; and might they have availed themselves of these alternatives either in the presence or in the absence of collective bargaining?

*The Assumption of Perfect Competition.* If we assume perfect competition in the labor markets for basic steel, we can presume that "a shortage would cause the bidding up of wages in the absence of collective bargaining. . . ." However, the assumption of the existence of conditions which would ensure perfect competition in the labor markets for production workers in basic steel in the absence of unionism is unwarranted. One writer noted that "a large company . . . adopt[s] and follow[s] a uniform policy for all its plants," and that at least the major companies tend to engage in "uniform behavior on labor conditions."<sup>16</sup> In addition to the policies of the union and the relatively large size of U. S. Steel and its consequent ability to assume its historic role of wage leader, he attributes these phenomena to the fact that steel mills typically employ a relatively large proportion of the labor of the types they require in the local labor markets involved. Where there is only one steel company in a town, "they are either the one company in a one-company town or one of a handful of large employers in a somewhat bigger community." Most of the industry's tonnage is produced in the large steel centers of the country; however: "When two steel plants are part of the same labor market, a wage increase by one certainly affects the other." And while steel employers compete with other industries for skilled maintenance workers and for common labor, the majority of the skilled and semiskilled jobs are unique to the industry<sup>17</sup> and are usually staffed by promotion.

This writer is describing monopsonistic or oligopsonistic labor markets. Now if a labor shortage exists in such a market it cannot be presumed that removal of an institution (in this case, the trade union) responsible for holding the wage rate at the level with which the shortage is associated would be followed by a "bidding up of wages." The new wage (and the new level of employment) would be determined by the relative positions of the demand curve for labor and the marginal-cost-of-labor and labor-supply functions; and the new wage might well fall below the union wage, even if at the latter wage there existed excess demand due presumably to an upward shift in the demand curve. Moreover, even immediately following a subsequent rise in the negotiated wage, the higher union wage could still be associated with some excess demand; this, however, does not appear to have been the case. Thus if "shortages existed soon after wage increases were negotiated," this

<sup>16</sup> Robert Tilove, *Collective Bargaining in the Steel Industry*, Philadelphia 1948, p. 31.

<sup>17</sup> *Ibid.*, pp. 31-32.

should not be taken as necessarily "indicating that negotiated rates at these times were probably below those which would have prevailed in the absence of bargaining." They might have been, but we cannot assess the probability involved solely from the evidence (a) that increases in employment occurred, (b) that price increases failed to eliminate product rationing, and (c) that intermittent local labor shortages were reported, primarily in the Chicago and Youngstown areas (although to what extent such shortages occurred among production workers in basic steel specifically is not known). In this context, the reporting of "labor shortage" might have signified only that employers wished to hire at the union rate more labor than was available at that rate. It did not necessarily signify that multiplant companies would have been willing to raise the wages of the existing forces in all their mills in an attempt to employ additional workers in some mills in those localities where labor was tight at the time, or even that they would have been willing to raise the local basic rate alone.

*Some Dynamic Considerations.* Even if the wage level at which the local market could be cleared is assumed to have been above the union wage, it need not follow that, in the absence of collective bargaining, a labor shortage would have resulted in a wage increase. When the dominant employing units expect that the short-run supply curve of labor in the local labor market will shift to the right or that the demand for labor will be reduced in the near future, it is unlikely that what is regarded as a temporary shortage would touch off a wage increase. The period July 1946 to at least August 1947 was cited by Rees as having been characterized by serious local labor shortages in the Chicago-Gary and Youngstown areas. Now some of these reported labor shortages were due to shortages in housing facilities near the plants; in one instance, it was specifically reported that, "The number of workers recruited [from the Pittsburgh region] was limited, apparently, only by the availability of housing at the plant site."<sup>18</sup> And, during the same postwar period, some reported shortages were attributed to the availability of unemployment compensation benefits to war-weary civilian workers and returning servicemen; while the Chicago steel mills were trying to fill about 3,300 jobs (mostly unskilled), 135,000 individuals in the area were reported as unemployed—and over 50,000 returning servicemen were expected.<sup>19</sup>

If the steel employers had had no union to contend with in 1946-47, would they really have responded to such shortages by raising wages sufficiently to have eliminated them? If not, then the reported existence of shortages under collective bargaining is not sufficient to create the presumption that collective bargaining held wages down. Nor can it be held to imply that unionized employers were not paying wages higher than they would have been willing to pay in the absence of the union but with all other conditions remaining the same. If we admit that the employer, under nonunion conditions, might tolerate some excess demand which he regards as temporary, then we cannot tell whether the shortage actually observed under collective bargaining is

<sup>18</sup> *The Iron Age*, Nov. 21, 1946, p. 136.

<sup>19</sup> *Ibid.*, Aug. 12, 1946, p. 44; Aug. 29, 1946, p. 124.

larger or smaller than the shortage which he would find tolerable under non-union conditions.

Relevant to the latter consideration is Hicks' suggestion that employers who have reason to believe that the supply curve of labor to their industry is considerably more elastic in the long run than in the short would hesitate to raise the wage rate sufficiently to cover a momentary shortage and later to reduce it. They would be restrained by their fear of "cheating the expectations" of entrants to the trade.<sup>20</sup> Employers in the U. S. steel industry might well have been similarly motivated in the period between August 1918 and August 1923 (when the 8-hour day was introduced). With the exception of the depression of 1920-21, this was a period of chronic labor shortage in the industry, unrelieved by the five wage increases which were either too little or too late to remove the specific shortages which had provoked them.<sup>20a</sup> Two months after the ineffective wage increase of April 1923, *The Iron Age* approvingly quoted an admonition from W. I. King to "Raise wages as little as possible, thus avoiding the necessity of severe wage cuts at a later date"; and it added, "Advancing wages that must afterward come down causes trouble and it is human nature that it should be so. We are dealing with men as they are, not as they would be if the Golden Rule were their sole guide to thought and conduct."<sup>21</sup>

Both Hicks and King presumably assumed that the employers whom they had in mind were able to act on their long view; however, the employers' ability to do so implies in turn a departure from perfect competition on their side of the labor market. This condition, as we have argued, is satisfied in the case of basic steel. But now consider the assumptions underlying the argument that employers in steel, if there were no union, would set a wage high enough to eliminate what they regard as a temporary shortage and without regard to a foreseeable future excess supply with which that wage might be associated. This argument assumes either that such employers act irrationally in terms of their long-run situation or that the degree of competition among them is so high that they (literally) cannot help themselves. Hence, as in

<sup>20</sup> See J. R. Hicks, "Economic Foundations of Wage Policy," *Econ. Jour.*, Sept. 1955, LXV, 404.

<sup>20a</sup> In 1920 local labor shortages were reported and persisted from April to September; these followed a wage increase in February and were terminated before the next rise in wages, which did not occur until May 1921. In 1922, shortages which were reported from the beginning of the year were not responded to by a wage increase until September, and this increase did not have the effect of stopping complaints of labor shortage. Furthermore, a wage increase of 11 per cent in April 1923 failed to alleviate a severe and persistent shortage of labor; as a result of this U. S. Steel inaugurated the basic 8-hour day in August with a compensating wage rise. The latter move did put an end to the persistent labor shortage, incidentally, which clearly reveals the prior effect of the industry's policies with respect to hours of work upon its ability to attract and retain native-born labor.

<sup>21</sup> *The Iron Age*: Apr. 15, 1920; Sept. 2, 1920; Jan. 26, 1922; May 4, 1922; June 29, 1922; Oct. 12, 1922; Nov. 9, 1922; Dec. 28, 1922; May 31, 1923; June 21, 1923. U. S. Steel also reported labor shortages during the second half of 1919; the subsequent increase occurred on Feb. 1, 1920.



the previous section, we are led to note that the view that labor shortage-cum-bargaining implies a restraining effect of unionism upon wage increases leans heavily on the assumption that perfect competition would prevail in the labor market in question in the absence of unionism.

*Overtime—An Available Remedy.* Presumably neither Rees nor we would deny that employers who believe it to be in their short-term interest to raise employee compensation to a given level would hesitate to do so if they could subsequently reduce compensation without fear of employee resistance. We might further agree that workers would tend to oppose an attempt to reduce their basic rate of pay and that knowledge of this would make employers unwilling to raise that basic rate in the first place. But there is more than one way to skin this cat, and it is available to union as well as to nonunion employers. By paying overtime premiums during transitional periods of worker shortage, employers could expand their employment of man-hours (a) without having to pay a higher rate for the straight-time hours throughout the enterprise, (b) without contravening either the letter or spirit of their fixed-term agreements, and (c) without courting the alleged peril of being prevented by the union from reducing average wages in response to a subsequent decrease in demand for labor.

Rees cites the specific instance of the Youngstown Sheet and Tube Company whose vice-president in charge of industrial relations testified before a Senate committee that, in August 1947, his company was short 1,000 workers of its normal complement of 24,000 in the Youngstown and Chicago-Gary areas. But this official added, "We do make up that thousand men by employing men overtime, but at the same time the efficiency is not there."<sup>22</sup> Whether or not this company would have been any more willing to increase its unit labor costs by raising its straight-time wage rate and its outlays on employee selection and training sufficiently to increase its work force by 4 per cent than it was to increase them by paying an overtime premium and incurring loss of efficiency is not known. However, we do know that throughout the period 1946-1948 basic steel was not, in the aggregate, on an overtime basis. In the ten strike-free months of 1946, the average work week was 37.4 hours; in 1947, 39 hours; and in 1948, 39.5 hours. (In all manufacturing, the length of the work week was 40.4 in 1946 and 1947 and 40.1 in 1948.) During the wartime period, 1942-1945, however, the work week in basic steel exceeded 40 hours, as it did—although to a much lesser extent—in 1951 (40.9), 1953, 1955, and 1956 (all 40.5).<sup>23</sup> Apparently the industry was unwilling to pay premium wages for overtime—according to one report of a labor shortage at the end of 1945, "mosily because it feels that only on a straight-time basis can it meet its present costs. This has meant the elimination of overtime." Between 1945 and 1946, when the work week declined by 9.1 hours, total

<sup>22</sup> U. S. Congress, Senate, *Steel Supply and Distribution Problems Affecting Smaller Manufacturers and Users*, Hearings before the Special Committee to Study Problems of American Small Business, 80th Cong., 1st Sess. (1947), p. 2019.

<sup>23</sup> The length of the average work week in 1942 was 40.2 hours; in 1943, 44.3; in 1944, 46.3; and in 1945, 44.1. The foregoing are BLS data.



man-hours worked in the entire steel industry declined, while the number of hourly rated employees rose by over 4 per cent.<sup>24</sup>

Thus an alternative to raising the base rate existed, but it was not significantly used, apparently because it cost too much. How, then, do we interpret those reports of local labor shortage? It is possible that they signified merely that, while employers would have been willing to hire more labor than was available to them at the going rate, they would not have been willing to raise the average rate sufficiently to pay overtime. Indeed, in this context, it is even possible that reports of local shortages might have signified in some cases that the employers involved wanted to hire more labor, but only at a lower wage rate. For that is what would be implied if their unwillingness to pay overtime meant that they would also have been unwilling to raise their total hourly payroll costs by the amount necessary to defray the additional costs of recruitment, selection, training, etc. involved in the expansion of their work forces.

In any event, the phenomenon of reported local labor shortages combined with employer unwillingness to raise the average rate of pay by as much as the offer of time-and-a-half would necessitate is consistent with another hypothesis advanced by Rees as applicable to this problem.<sup>25</sup> This hypothesis is that the industry's policy of setting steel prices below the levels at which the product markets would be cleared depressed the level of demand for its production labor. But now one might question his further claim that the union was in part responsible for this price policy.

*Holding Down Prices and Wages.* We have suggested that, in the pre-union era of the basic steel industry, the fear of future unionism (or, as it used to be called, "labor unrest") led employers to exercise restraint in granting wage increases in response to labor shortages which they believed to be temporary. Now, in the postwar period under consideration, the expectation of a future tapering off in demand was widely held by the industry's leadership.<sup>26</sup> Therefore, even if the steel union had been nonexistent in 1946-48, it appears more likely that the industry would have exercised some moral restraint in its wage-price policy than that it would have thrown caution to the winds. Hence one cannot easily accept the view that the substance of unionism contributed independently to the restrained behavior of the industry in 1945-48 if the same type of behavior would have been elicited by its shadow.

Of course one might maintain that the threat of unionism would exert a lesser degree of influence than the presence of unionism, but it would be at

<sup>24</sup> *New York Times*, Dec. 15, 1945, p. 12; *The Iron Age*, Aug. 15, 1946, p. 92. The *Times* report also noted that, despite the incomplete success of its recruitment campaign at the time, U. S. Steel was apparently making no effort to hire or retain "any considerable number of women, who held down 20 percent of the jobs at the Irvin works during the war."

<sup>25</sup> This theory of union accountability for price-wage inhibition in administered price industries was originally set forth by W. A. Morton, "Trade Unionism, Full Employment and Inflation," *Am. Econ. Rev.*, March 1950, XL, 18.

<sup>26</sup> U. S. Senate, *Steel Supply and Distribution Problems*, op. cit., pp. 2002-3, 2005, 2007-8, 2010-11, 2016.

least as easy to argue to the contrary. In the first place, employers unionized at the outset would have to reckon with a higher probability of a strike for higher wages during the period of labor shortage than would nonunion employers. Thus, if unionized employers wished to secure a future wage rate with which no excess supply of labor would be associated, they would presumably have to choose between incurring a strike in good times and in the near future in order to hold wages below the levels at which markets could currently be cleared, and incurring a strike to reduce wages in some possible period of future labor surplus. Since the loss of business from a strike would be less and the prospects of their winning would be greater under the latter alternative, that alternative would presumably be preferred. Moreover, a firm like U. S. Steel, to which a policy of present restraint probably commended itself with particular force under nonunion conditions, found that unionism eliminated certain exceptional circumstances which had prevailed in the past. Since U. S. Steel had traditionally sought to play the role of wage and price stabilizer within the industry under nonunion conditions, it lagged behind other firms in reducing wages during the downswings of 1920-21 and 1929-32. A policy of wage restraint during periods of labor shortage might well have appealed to such a "leader" under nonunion conditions since it tended to minimize its differential losses during ensuing downswings. But once the union was able to inherit the stabilizer's mantle, the dominant firm no longer had such a strong private stake in holding wages below the levels at which they would have cleared the labor markets.

Of course, one need not rely upon the theory of unionism as a price depressant to account for the industry's conservative pricing policies during this period. In the light of the industry leaders' conviction (mentioned above) that postwar operating rates were abnormally high, their consequent reluctance to expand capacity in the face of strong public pressure to do so, their fear of price controls and their frequent desire to ration in the interests of their traditional customers, wage-price restraint is understandable enough.<sup>27</sup>

Now price restraint, whatever its cause, tended to hold down the rate of increase in the demand for production labor in basic steel; thus one need not abandon Rees' hypothesis that "the smaller increase in steel prices" in 1939-48, as against 1914-20, was a factor which helped to account for the smaller increase in money wages during the latter period. But if we refuse to agree that the 1939-48 price increase would have been greater in the absence of collective bargaining, we are certainly not obliged to conclude that the 1939-48 increase in money wages would also have been greater in the absence of collective bargaining. Rees' data (p. 398) reveal that the rise in steel wages relative to steel prices was much greater in 1939-48 than in 1914-20; in the earlier period base prices of finished steel rose by 144 per cent while wages rose 148 per cent, whereas in 1939-48, a price rise of 61 per cent was accompanied by a wage increase of 99 per cent. For reasons presented in this section, we regard it as improbable that the union was instrumental in holding prices down during the latter period; for reasons presented in Section

<sup>27</sup> *Loc. cit.*; *The Iron Age*: Jan. 29, 1948; June 10, 1948; Sept. 21, 1948; Oct. 7, 1948.

II above, we regard it as more likely that the union was a factor in forcing up the ratio of wage increase to product price increase in the second period.

#### IV. *Recent Developments: 1948-1956*

It is of interest to determine how the organized workers in basic steel fared since September 1948, which is the terminal date of Rees' study and the month in which their average hourly earnings reached their peak in the immediate postwar period. Limitations of space make it impossible to catalogue the changes in the conditions of employment secured in the six separate negotiations in the period 1948-56.<sup>28</sup> It should be noted, however, that changes in the geographical and occupational wage structures in the industry and in so-called "fringe benefits" (including pensions and insurance, paid holidays, shift differentials, and the Supplementary Unemployment Benefit Plan), as well as in basic wage rates were negotiated under collective bargaining during this period. This brief account, however, will consider only changes in the level of average hourly earnings, which, of course, fail to reflect some of the changes in the terms of employment other than basic rates. On the other hand, average hourly earnings have tended to increase more rapidly than contractual rates in this period; this is due in great part to the inclusion of an increasing proportion of the work force under incentive pay plans—a process, incidentally, in which the local unions involved have played a significant role.

Table II compares increases in average hourly earnings in basic steel with wage increases in some of the lightly organized industrial categories identical with or corresponding closely to those employed by Rees (those for which data have been continuously available) and in all manufacturing. The table reveals that basic steel's standing in the group including itself and the 9 lightly organized (in 1946) trades improved markedly between 1948 and 1956. Between 1939 and 1948 and between 1945 and 1948, 4 of these trades had obtained increases in average hourly earnings greater than those secured in basic steel. (Some of the factors contributing to this result were offered in Section I above.) But between 1948 and 1956 only the ice-cream workers received greater increases than the steel workers—a phenomenon most appropriate to the democracy's cold-war economy.

We need not repeat our early caveats concerning the interpretation of the results of such comparisons. They are consistent with the hypothesis that unionism exerted an independent impact on the level of steel wages in any or all or none of the periods considered. However, certain additional information makes plausible the hypothesis that the union probably did exert an independent influence on the level of money wages in basic steel after September 1948. In the first place, while the increase in basic steel production (33 per cent) over the period 1948-55 was about the same as the increase in industrial and manufacturing production as well as in gross national product in 1956 prices (34, 36, and 33 per cent respectively), employment in basic

<sup>28</sup> See U. S. Dept. of Labor, BLS Report No. 106, *Wage Chronology: United States Steel Corporation, 1937-1955*, Washington, 1956.

TABLE II.—ORIGINAL LEVELS AND RELATIVE CHANGES IN AVERAGE HOURLY EARNINGS, BASIC STEEL AND OTHER SELECTED CATEGORIES

	(1) Average Hourly Earnings 1939	(2) 1948 to 1939	(3) 1948 to 1945	(4) Ratio 1956 to 1948	(5) 1956 to 1939
Basic Steel	.845	198	139	151	299
Crude Petroleum	.873	196	146	145	284
Nonmetallic Mining	.550	232	145	150	349
Confectionery	.492	214	137	142	305
Nonalcoholic Beverages	.556	192	131	147	282
Ice Cream	.626	187	142	157	294
Power Laundries	.417	198	124	127	252
Cleaning and Dyeing	.490	201	128	128	257
Wholesale Trade	.715	191	134	147	281
Retail Trade	.536	203	142	145	293
Total Manufacturing	.633	219	133	143	313

Source: Original data in *Monthly Labor Review*.

steel increased considerably less than employment in manufacturing and total civilian and nonagricultural employment—the increases being about 1.4 per cent in steel, 8.1 per cent in manufacturing, 6.4 per cent in civilian employment, and 12.4 per cent in nonagricultural employment. This suggests that increases in productivity and/or prices enabled the industry to expand output without placing its labor markets under heavy stress. Between 1948 and 1955 output per production-worker man-hour increased by slightly under 28 per cent. Since a recent BLS bulletin notes that factors making for retardation in the rate of increase in physical productivity were especially in evidence in the early years following the second world war,<sup>29</sup> it might be presumed that physical productivity increased somewhat more rapidly in 1948-55.

Moreover, the industry overcame its postwar reluctance to expand capacity; while total ingot and castings capacity decreased slightly between 1945 and 1948, capacity increased by over one-third between 1948 and 1955-56, although operating rates remained within the 90-95 per cent range.<sup>30</sup> Nor is the industry's post-1948 price history inconsistent with the view that it abandoned its postwar pessimism. Finished steel prices rose almost three times as

<sup>29</sup> U. S. Dept. of Labor, BLS Bull. No. 1200, *Man-Hours per Unit of Output in the Basic Steel Industry, 1939-1955*, Washington 1956, pp. 3-4, 8.

<sup>30</sup> Am. Iron and Steel Inst., *Annual Statistical Report, 1955*, New York 1956, p. 53.

rapidly as wholesale prices other than farm products and food between September 1948 and December 1956; in contrast, steel prices had risen very little faster than these wholesale prices between August 1945 and September 1948 and thus lagged considerably behind the latter over the entire period 1939-September 1948.<sup>31</sup>

Taken together, the increases in productivity, capacity, and prices might support the view that the industry was more able and would have been more willing (in terms of its expectations) to grant wage increases after 1948 than before, provided that it was obliged to do so. The relatively modest increases in employment combined with the relatively low quit rate in basic steel suggest that the industry was not under heavy market pressure to grant the wage increases which it did put into effect. On the other hand, the fact that—in addition to the installation of the pension and insurance plans in 1949—two of the five wage increases granted after 1948 were associated with strikes and that the 1955 settlement followed a stoppage of a few hours suggest that the industry was under real pressure from the union during this period.

LLOYD ULMAN\*

<sup>31</sup> *Mo. Lab. Rev.*; Jules Backman, "Steel Prices, Profits, Productivity and Wages," in *Steel and Inflation—Fact vs Fiction*, New York: U.S. Steel Corp., 1958, p. 104.

\*The author is professor of economics at the University of Minnesota, currently on leave at Harvard University. He acknowledges with gratitude the kindness of Sumner H. Slichter of Harvard University, Jack Stieber of Michigan State University, John H. Kareken of the University of Minnesota and Otis Brubaker of the United Steelworkers of America, who read an earlier draft of this paper and made many valuable suggestions. He wishes to acknowledge the generosity of the Social Science Research Council, a faculty research fellowship of which he held during the period in which this paper was written.

### Reply

I am pleased that after seven years Professor Ulman still considers my article of sufficient interest to warrant his careful analysis of it. However, I cannot agree that he has weakened my conclusions. In this reply, I shall examine in turn his discussion of the three main strands of my argument. Before doing so, however, I should like to restate the conclusion of my article. I concluded that wages in basic steel during the period 1945-48 were about what they would have been in the absence of collective bargaining. It seems somewhat more likely that collective agreements held wages down slightly in some localities than that they raised wages, but such effects if present would have been small. If Ulman feels that I implied that collective bargaining depressed wages substantially—say 10 per cent—he has misread me. If his own conclusion is that it raised wages substantially—say 10 per cent—I do not think the evidence supports him.

#### I. The Interindustry Wage Comparison

For the purpose of estimating union effects, wages in the steel industry should ideally be compared with wages in industries that are like steel in every important respect except that they are unorganized. There are, of course,

no such industries. But despite their limitations, interindustry comparisons are of some value. Ulman suggests that this value might be increased by the explicit control of differences among industries in such variables as productivity, initial levels of earnings, and the sex composition of employment. Since his stated purpose is to challenge my conclusions, and not merely to criticize my methodology, he has not pursued these issues far enough. One must ask in which direction and by how much the failure to control for these variables affects the results. To throw light on this question, I shall use such additional data as I have been able to assemble on short notice.

Productivity was considered in my original article; I constructed an index of output per man-hour in the steel industry before the BLS index for this period was available. However, data for all manufacturing were not then available for comparison.

A comparison of changes in output per man-hour in steel with available data of other industries is shown in Table I.

TABLE I.—INDEXES OF OUTPUT PER MAN-HOUR, SELECTED INDUSTRIES, 1948  
(1939=100)

Basic steel	
Original estimate	130 <sup>a</sup>
BLS series	126 <sup>b</sup>
All manufacturing	111 <sup>c</sup>
Confectionary	115 <sup>d</sup>
Ice cream	139 <sup>e</sup>

<sup>a</sup> A. Rees, "Postwar Wage Determination in the Basic Steel Industry," *Am. Econ. Rev.*, June 1951, XLI, 394.

<sup>b</sup> Computed from Joint Economic Committee, *Productivity, Prices, and Incomes*, 85th Cong. 1st Sess., p. 220.

<sup>c</sup> Computed from *ibid.*, p. 148.

<sup>d</sup> *Productivity Trends in Selected Industries*, BLS Bull. 1046, Washington 1951, p. 8.

<sup>e</sup> *Ibid.*, p. 11.

It is unfortunate that BLS productivity indexes have been published for only two of the nonunion industries in my wage comparison, one of which (ice cream) is an exception to the general wage pattern. However, the large difference between the productivity indexes for basic steel and all manufacturing suggests that output per man-hour rose more in steel than in most other industries during this period. Previous studies have shown, as one would expect, a positive relation among industries between changes in output per man-hour and changes in average hourly earnings.<sup>1</sup> It thus seems clear that productivity differences among industries were a factor tending to raise relative wages in steel, and that explicit control for differences in productivity would have strengthened my conclusions.

It was not possible for me to use the technique used by Levinson and by Ross of making wage comparisons among industries grouped by initial wage

<sup>1</sup> See, for example, J. W. Garbarino, "A Theory of Interindustry Wage Structure Variation," *Quart. Jour. Econ.*, May 1950, LXIV, 298-99.



level. As Ulman points out, only one weakly organized industry had an initial wage as high as that of steel. However, Levinson, who applied this technique to all unionized industries, reached conclusions similar to mine. Of the period 1941-47 he writes "All comparable wage-earner groups in the manufacturing, extractive, public utility, and construction industries obtained approximately equal wage increases, regardless of union strength."<sup>2</sup>

Ulman makes a good point about the sex composition of employment in the industries I compared. Steel is almost exclusively male, while some of the comparison industries employ many women. Moreover, sex differentials narrowed to varying extents within almost all industries between 1939 and 1948.<sup>3</sup> Thus I tended to overstate the rise in earnings of males in other industries relative to earnings of males in steel. However, allowing for this effect does not seem to alter my results materially, as shown in Table II.

Table II is an expansion of Table IV of my original article. A column has been added showing the per cent of female employment in each industry in 1939. Estimates of wages increases for males only have been made for the three industries for which data are available on average hourly earnings by sex for 1939 and 1947 or 1948. The wage increase figures have been separated into two groups: Column 4 shows the wage increases in the seven industries with more than 20 per cent female employment in 1939. Column 5 shows the new estimates for males only in three industries, and the original data for industries that were more than 80 per cent male in 1939. A comparison of the wage increase in steel with the other increases shown in column 5 does not lead to conclusions different from those of the original table.

Whether wage controls were more effective in steel than in the comparison industries does not seem to me to be a relevant issue. The terminal date of my comparisons is long after the end of wage control and there was ample time for wages to return to levels determined by other forces.

I agree that wage increases won by unions often influence the wages of non-union employers. It is this that requires that the union and nonunion earnings considered be for establishments that are not too similar. For example, one obviously could not compare union and nonunion steel plants. However, in the industries selected as weakly organized, the threat of union expansion during this period seems weak enough so that it could not have appreciably affected average earnings in the industry.

Ulman's major criticisms of the interindustry comparisons work in opposite directions. When they are all taken into account as far as possible, the comparisons still suggest that wages in basic steel from 1939 to 1948 rose by approximately as much as they would have risen in the absence of a union.

## II. Comparisons Between Two War Periods

Ulman argues that the conclusions I draw from comparing the two war periods are invalid in part because the increase in steel employment was larger

<sup>2</sup> H. M. Levinson, *Unionism, Wage Trends, and Income Distribution 1914-1947*, Michigan Bus. Stud. Vol. X, No. 4, Ann Arbor 1951, p. 67.

<sup>3</sup> See the data on average hourly earnings by industry and sex in various issues of the National Industrial Conference Board *Manag. Record*.



TABLE II.—COMPARISONS OF CHANGES IN AVERAGE HOURLY EARNINGS  
SELECTED INDUSTRIES, 1939-1948

Industry and Sex	Per Cent Female 1939 <sup>a</sup>	Per Cent Union 1946 <sup>b</sup>	Average Hourly Earnings September, 1948 as Per Cent of 1939 Average	
			More than 20% Female	Less than 20% Female
(1)	(2)	(3)	(4)	(5)
Basic steel	0.9	80 to 100		199
Crude petroleum	3.2 <sup>c</sup>	20 to 39		196
Nonmetallic mining	1.7 <sup>c</sup>	20 to 39		233
Cotton textiles	38.4	20 to 39	289	
Males	—	—		279 <sup>d</sup>
Silk & rayon textiles	49.7	20 to 39	281	
Males	—	—		270 <sup>e</sup>
Confectionary	66.5	20 to 39	221	
Males	—	—		204 <sup>f</sup>
Nonalcoholic beverages	2.8	20 to 39		193
Butter	14.4	20 to 39		223
Ice cream	86.2	20 to 39	187	
Power laundries	64.5 <sup>c</sup>	20 to 39	199	
Cleaning and dyeing	30.3 <sup>c</sup>	20 to 39	197	
Wholesale trade	19.3 <sup>h</sup>	1 to 19		193
Retail trade	34.1 <sup>i</sup>	1 to 19	203	

<sup>a</sup> Computed from data for wage earners, *Census of Manufactures*, 1939, Vol. I, pp. 70-85 except as noted.

<sup>b</sup> For sources and notes, see Rees, *op. cit.*, p. 399.

<sup>c</sup> Data are for April 1940 and include salaried workers, computed from *Census of Population*, 1940, "Labor Force, Sample Statistics, Industrial Characteristics."

<sup>d</sup> Male earnings in 1939 were estimated from (a) ratios of male to female average hourly earnings in the industry "cotton-North" in June 1939, computed from the data of the National Industrial Conference Board; (b) the ratio of female to male employment shown in column 2; and (c) BLS average hourly earnings in the industry "broad-woven cotton textiles" for all of 1939. Male earnings for 1948 were similarly estimated, using NICB data for June 1948; BLS data for September 1948; and the 1939 employment weights by sex.

<sup>e</sup> Same method as in cotton, using the NICB industry "silk" in 1939 and "silk and rayon" in 1948.

<sup>f</sup> Same general method as in cotton. Ratios of male to female earnings for 1939 were computed from *Mo. Lab. Rev.*, Mar. 1940, L, 629, based on BLS data for twelve states. The ratio applied to the 1948 wage for both sexes is for "candy and chocolate" for January 1947, from BLS Industry Wage Studies Bull., Wage Structure Ser., No. 55. A very similar ratio for the confectionary industry in Illinois is given in *Illinois Labor Bull.*, Jan. 1948.

<sup>g</sup> *Census of Business*, 1939, Vol. III, "Service Establishments," p. 59.

<sup>h</sup> *Ibid.*, Vol. II, "Wholesale Trade," p. 15.

<sup>i</sup> *Ibid.*, Vol. I, "Retail Trade," p. 21.

in the first than in the second world war. For "illustrative purposes" he estimates the percentage increase in wages for 1939-48 on the assumption that it bore the same relation to the percentage increase in employment as in 1914-20. This yields the three estimates of the 1939-48 wage increase of 71, 66, and 62 per cent. The actual increase exceeds the middle estimate of this set by 32 per cent. The illustration seems to suggest that without the

union wages might have increased only by the estimated percentage, and that the union won increases one-third larger than would have been obtained in its absence. Such power would equal that of strong craft unions under highly favorable circumstances. Later Ulman warns that the illustration cannot be used in this way. One is left to wonder whether it can be used at all.

The same technique could be applied to estimate the increase in earnings in all manufacturing. Employment of wage earners in manufacturing rose 28 per cent from 1914 to 1919<sup>4</sup> and average hourly earnings rose 114 per cent.<sup>5</sup> From 1939 to 1948 production-worker employment in manufacturing as reported by the Bureau of Labor Statistics increased 52 per cent. On the assumption that the increase in earnings bore the same relation to the increase in employment from 1939 to 1948 as it did from 1914 to 1919, we obtain an "estimated" increase in earnings for 1939-48 of 212 per cent! The actual increase was only 113 per cent according to BLS data. But the extent of unionization in manufacturing was much greater in 1939-48 than in 1914-19. Does the calculation therefore suggest that unionization held down earnings in manufacturing during the second world war? I think not.

The difficulty with Ulman's technique is that it assumes that the supply schedule of labor to the steel industry was (a) the same in both periods and (b) highly inelastic in both—that a wage increase of 147 per cent was needed to expand employment 54 to 62 per cent. Ulman recognizes and discusses the first of these assumptions, but not the second. Viewing these money-wage increases from the supply side, we must think of a rising level of the supply schedules and not of movements to the right along steeply sloping industry schedules. In other words, the wages an industry must pay during an inflation depend only in small part on the extent to which that industry expands, and in large part on the tightness of the general labor market. Inelastic supply to an industry must result from the specialization of labor to industries, and this must be more true of skilled than of unskilled workers. Ulman's reasoning thus suggests that the wages of skilled workers will increase relative to those of unskilled workers during periods of rapid expansion of employment. What we observe, of course, is the reverse.

In comparing the ability of steel mills to attract labor during the two wars, Ulman makes too much of the differences between the periods and not enough of the similarities. The principal difference was in hours of work. This is exaggerated by talking about the 12-hour day as if it were the standard work day for all steel workers during the first world war. Of the 278,000 wage earners in basic steel in 1914, only 55,000 had a scheduled work week of 72 hours or over. The work week of 194,000 workers was 60 hours or less—the 10-hour day then common in most other industries.<sup>6</sup> Considerable progress had been made in shortening the work week since 1909, in part as a consequence of the Senate inquiry of 1911 which Ulman cites.

<sup>4</sup> Computed from Solomon Fabricant, *Employment in Manufacturing, 1899-1939*, New York 1942, p. 214.

<sup>5</sup> Computed from BLS data given in *Historical Statistics of the United States, 1789-1945*, p. 67.

<sup>6</sup> *Census of Manufactures, 1914*, Vol. II, pp. 205 and 223.

Many jobs in steel mills during both wars had other disadvantages that Ulman does not stress. They were hot, heavy, or dirty, and often all three. These factors were mentioned in accounts of recruiting difficulties in the period 1939-48.<sup>7</sup>

### III. Shortages and Expectations

Ulman seems to believe that I assumed perfect competition in the steel labor market in the absence of unions, though I stated no such assumption. The term "perfect competition" can unfortunately always be used to deride economic analysis because it is obvious that no market, and certainly no labor market, can be truly perfect. I agree fully with George Seltzer who, in a companion study to mine, concluded that there was wage leadership in the basic steel industry long before the union. But he added "It should not be inferred from the preceding sections that the USA-CIO has had no influence upon wage leadership and wage uniformity in basic steel. Quite the contrary is true."<sup>8</sup>

There are several reasons for believing that the wage leadership of the period 1914-20 permitted more complete adjustment of wages to labor shortages than the collective bargaining of 1939-48. First, the leadership of the earlier period applied largely to the timing of wage increases, and even there was not complete. It permitted substantial geographical differences within the North, as well as some local differences, in the amounts of increases and the levels of wages.<sup>9</sup> The geographical differences were well suited to distinguishing between markets where labor was scarce and those where it was not. In contrast, after May 1946 the only remaining geographical differential was that between the North and the South. Second, in the earlier period there was no barrier to increasing wages for particular occupations not specialized to steel in which there were labor shortages, though such increases announced by one producer would probably have been followed by other producers in the locality. After January 1947 all jobs were classified in a contractual system of skill differentials. A wage increase in an occupation where there were labor shortages could not be made without also increasing wages in other occupations in the same job class where labor was plentiful. Finally, the practices of the earlier period permitted more frequent adjustment of wages; wages in some mills were raised as often as three times in one year.

Ulman states that the five wage increases between August 1918 and August 1923 were "too little or too late to remove the specific shortages which had provoked them." This suggests that he has evidence of labor shortages immediately following these increases, but unfortunately this evidence is not presented. If employer collusion holds down wages in the absence of unions and unions reduce this effect, the evidence of labor shortage should be clearer for 1918-23 than for 1945-48. There is, however, clear evidence that the in-

<sup>7</sup> See, for example, U.S. Employment Service, "Labor Market Information," Indus. Ser. No. 33-1, Curr. Suppl., Apr. 1946 (mimeo.).

<sup>8</sup> "Pattern Bargaining and the United Steelworkers," *Jour. Pol. Econ.*, Aug. 1951, LIX, 324.

<sup>9</sup> These statements are based on unpublished tabulations of common labor rates in eight steel mills in seven localities, which were kindly furnished to me by John T. Dunlop.

dustry did not always follow the advice of W. I. King against wage increases that must later be rescinded. Wages in some mills were cut on as many as four different occasions during the recession of 1921, and these cuts were not fully restored by the increases of 1922-23. There have been no such wage cuts since the second world war, though recent recessions, of course, have been milder than that of 1921. Although these differences between these two periods have many causes other than collective bargaining, certainly collective bargaining contributes to them.

I could not include in my article all the evidence of labor shortages during 1946-48 collected in my study. All of it related specifically to production workers in basic steel, and it came from a variety of sources.<sup>10</sup> Evidence of unemployment in the whole labor force of the Chicago area is hardly relevant to this point. These unemployed included women, white collar workers, and others not suitable for steel employment.

Ulman emphasizes the availability of overtime as a way out of labor shortages. But this is a very expensive way out. Apart from any effect of longer hours on efficiency, overtime hours cost 50 per cent more than straight-time hours. The use of substantial amounts of overtime may thus be much more expensive than straight-time wage increases designed to meet particular shortages if these shortages are expected to persist for any considerable time. The failure to use substantial amounts of overtime work therefore does not contradict the evidence of shortages of labor at prevailing straight-time rates.

Ulman's final objection to my position concerns wage-price relations. Three main hypotheses have been advanced to explain the underpricing of steel and automobiles immediately after the second world war: (a) fear of government control or antitrust action; (b) fear that temporary price increases might help unions win permanent wage increases; and (c) the desire to build customer good will (steel) or strong dealer organizations (automobiles). Like Ulman, I believe the first of these to be the most important.<sup>11</sup> Since I know of no evidence that enables us to discriminate among these hypotheses, I cannot prove that the second one has any validity. But neither has Ulman proved that it has not. Pricing decisions in oligopolistic industries are complex, and need not be attributed to single causes. Decisions to sell at prices far below those that would clear the market require a lot of explaining, and all the hypotheses together seem barely adequate.

#### IV. Conclusion

Ulman's section on developments since 1948 I do not regard as relevant to the main point of my article. It suggests, however, a misunderstanding of the way in which my results can properly be generalized. Ulman seems to read my conclusion as implying that the United Steelworkers has always been a union without economic power. But this inference is unwarranted. Indeed, I specif-

<sup>10</sup> For further details, see my unpublished doctoral dissertation, "The Effect of Collective Bargaining on Wage and Price Levels in the Basic Steel and Bituminous Coal Industries, 1945-48," University of Chicago, pp. 38-42.

<sup>11</sup> See my article "Wage-Price Relations in the Basic Steel Industry, 1945-48," *Indus. Lab. Rel. Rev.*, Jan. 1953, VI, 193-205.

ically attributed the 1949 pension settlement to the power of the union. I also believe that the union won wage increases in 1937 that would not have occurred without it. A thorough study of the effect of the union on steel wages since 1948 would now be of great interest, and I can only guess within broad limits what it might show.

The general conclusion that my study does support is that collective bargaining *during periods of rapid inflation* is very unlikely to make wages rise faster than they otherwise would, and is quite likely to cause them to lag. This conclusion is now supported by additional evidence drawn from other industries, including the industries that were unionized in 1914-1920.<sup>12</sup> The conclusion is also supported by Levinson's study. He finds no union impact on wages in 1914-20 or in 1939-47, but does find an impact in the period of price stability from 1923 to 1929.<sup>13</sup>

These findings suggest that collective bargaining is not among the causes of rapid inflation. Whether it is among the causes of gradual inflation must still be regarded as an open question, though the burden of proof rests on those who feel that it is.

ALBERT REES\*

<sup>12</sup> See S. P. Sobotka, "Union Influence on Wages: The Construction Industry," *Jour. Pol. Econ.*, Apr. 1953, LXI, 137-43; and Elton Rayack, "The Effect of Unionization on Wages in the Men's Clothing Industry," unpublished doctoral dissertation, University of Chicago. Both of these studies show strong union effects on wages in periods other than 1914-20 and 1939-48.

<sup>13</sup> *Op. cit.*, pp. 47 and 66-67.

\* The author is associate professor of economics at the University of Chicago. He wishes to thank H. Gregg Lewis for helpful suggestions.

### Errata

The third sentence in paragraph three of the note "External and Internal Public Debt," by James M. Buchanan, in the December 1957 number of the *American Economic Review* (p. 995) should be changed to read as follows: "If a given state or community could be confronted with two alternative situations identical in all respects save that in one an internal debt service charge is present while in the other such a charge is absent, the *second* is obviously to be preferred."

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The Cambridge Economic Handbooks series under the joint editorship of C. W. Guillebaud and Milton Friedman is now being published in this country by the University of Chicago Press. *The Economics of Underdeveloped Countries* by P. T. Bauer and B. S. Yamey is the first volume published under the new arrangement. The review of the book in the March 1958 number of the *American Economic Review* (pp. 170-71) did not include the University of Chicago Press as the American publisher.

## BOOK REVIEWS

### General Economics; Methodology

*Economics in the United States of America: a Review and Interpretation of Research.* By RUTLEDGE VINING. (Paris: United Nations Educational, Scientific, and Cultural Organization. 1956. Pp. 62. \$1.00.)

Professor Vining's short "review and interpretation" of recent developments in economics in the United States is one of several publications by Unesco in its series *Documentation in the Social Sciences*, an international "charting of research activity in the social sciences." Although the foreword describes Vining's review as a "report," it is in fact a critical essay on methodology in economics. The principal subject of the essay is recent theoretical work on "game theory, statistical decision theory, the theory of linear-programming and the like." The text of the essay, at least the part of it that I think I understand, is that a "political economist must search with care in order to find within this work techniques or materials directly applicable to the study and analysis of economic problems of the 'classical' type."

Vining starts his review with the observation that "it is in the forms in which the problems are considered . . . that we may observe the trends of economic research." Economic problems, says Vining, are always problems of choice among alternatives. Two types of choice problems, however, must be sharply distinguished from each other. The first type involves choice among means for a well-defined end. Thus, for example, a business enterprise faces the problem of choosing among alternative rules of action that rule which will lead to a maximum of a specified kind of output from given inputs of productive services, or the rule that will minimize costs of a given output, or the rule that will maximize the net worth of the enterprise. The rule that will be chosen or "ought to be chosen" in a choice problem of this type depends, of course, not only on the well-defined objective to be attained but also on the restraints imposed by the physical universe and by the legal and regulatory institutions of the social economic organization.

Many of the recent theoretical developments—such as game theory, linear programming theory, information theory, and queueing theory—Vining states, have been helpful both in clarifying the logic of this type of problem and in solving substantive problems in the real world. Operations research, to which Vining devotes three pages in the second part of his essay, is an excellent example of a field whose subject matter has been explicitly limited by its professional practitioners to problems of this first kind: to understand and hence ultimately to modify and improve the operations of a group whose activities are directed toward a well-defined objective.

The "means-end" problem is essentially an engineering one. The tasks that have to be performed in solving such problems are accounting for alternative systems of rules of action, defining clearly the objective to be achieved, pre-



dicting the performance of alternative rules, and ranking the rules according to the goodness of their performance. "The specialization of any expert who tenders advice on problems of choice," Vining states, "lies in his ability to analyse special kinds of rules of action, and the statements which he makes as a specialist have reference to one or another of the above tasks."

Conventional economic theory, particularly those parts labeled the "theory of the household" and the "theory of the firm," often is couched in language that makes it sound as though it were designed for the purpose of solving problems of choice of this first kind. Vining takes pains to point out, however, that those who interpret conventional theory this way grossly misunderstand it. He emphasizes that the classical economic theory of the political economist does not inquire into how a firm or a household solves its problems of choosing among means for well-defined ends, and in particular is not concerned with advising them on how they "ought" to solve problems of this type. Instead conventional theory simply assumes that firms and households do select "optimum" rules; deduces from these "solutions" certain equilibrium conditions (equalities and inequalities among marginal rates of substitution); and builds on these a theory of the market.

That the traditional tools of economic analysis were not designed for the purpose of advising firms and households on how to manage their business affairs has been said often before. But it needs to be said again. Much of the dissatisfaction among "business" and "labor" economists with economic theory undoubtedly stems from their disappointments and frustrations in trying to apply economic theory to the problems of individual business firms, unions, and the like.

If the purpose of conventional theory is not that of assisting firms and households to manage their affairs "rationally," then what is its purpose? This brings us to Vining's second type of choice problem: "the individuals constituting a . . . society of free agents jointly choose the constraints and regulations which they impose upon their individual actions. The decisions made by a legislative body in the drafting of legislative acts are examples of choices of this . . . type."

A free society is a commonwealth of free individuals engaged in a more or less continual review of the performance of the laws of the society and continually striving by discussion to reach consensus on the choice of laws. A free society is not a task-oriented group controlled by some decision-maker who seeks best rules for managing the group toward some well-defined objective. (Vining, in a four-page comment in the second part of his essay, scolds "welfare economists" for couching welfare economics in language that makes it sound as though the problems of choice facing a society of free individuals were simply means-ends problems of the first kind. It is not clear whether he intended his criticism to go to the substantive propositions of "welfare economics" or only to the language used in some expositions of welfare economics.)

Conventional economic theory, of course, is not a theory explaining how a free society chooses its laws or ought to choose them. Vining is absolutely right, however, in emphasizing that the work of the political economist, the



tools of whose trade are conventional theory, has been directed toward informing discussion among free citizens on their choices among alternative laws. The task of the political economist has been and is that of predicting the consequences—for prices, rates of consumption, and the like—of alternative laws. It is in this sense then that classical economics has been concerned with problems of choice of the second kind.

The trade of the political economist is not the trade of the management or "efficiency" expert. The tools of the first trade were not designed to serve the second one, and, indeed, they have little usefulness for the second. In the same way there is no presumption that the tools of the efficiency expert will have much usefulness for the political economist. This double-edged proposition is an important one, particularly for a document such as this one sponsored by Unesco presumably addressed at least as much to economists in the universities and ministries of poor countries as to economists in the United States.

Intellectual tools have a habit, unfortunately, of not being neatly labelled. How does Vining, or any one else for that matter, know whether a recent theoretical development in "economics" is "designed" to serve the political economist or the operations researcher? Consider, for example, the recent literature on the problem of choice under risk and uncertainty. Many of those who have written in this field state explicitly that their contributions are designed to improve upon conventional economic theory, to make it more useful to the political economist. Should we not believe them? Language is no help here, for a contribution that sounds as though it is dealing with the first type of choice problem may, like the economic theory of the firm, be recast in language that is in the mode of conventional economic theory.

At this point Vining's methodological scheme breaks down and another test must be brought to bear: do the contributions enable the political economist to perform his tasks better? In fact this is the test that Vining applies to both game theory and "simultaneous equations econometrics." I am puzzled, therefore, by his treatment of game theory as work on the first type of problem—the choice of means for a well-defined end—while treating the "econometrics" approach to analysis of the business cycle problem as an example of poorly conceived work on the second type of problem—the problem of appraising an economic system.

I am even more puzzled as to Vining's purpose in the last 19 pages of his essay. In these pages he recounts the discussions in official circles, both in the 1920's and in recent years, of the problem of economic instability; and he reviews briefly and favorably the empirical work on the business cycle by Arthur F. Burns. He emphasizes the continuing ambiguity of the concept of "economic stability" and laments the poverty of political economy as an intellectual discipline in social discussion of the stability issue. At the end of the essay he expresses the belief that a useful theory of business-cycle phenomena, based on Burns' work, eventually will be developed.

H. GREGG LEWIS

*University of Chicago*

*An Economic Theory of Democracy.* By ANTHONY DOWNS. (New York: Harper & Bros. 1957. Pp. viii, 310. \$4.50.)

Dr. Downs applies to the study of democratic politics the methodology of model-building that is now familiar in economics. If political science is ever to evolve into a more theoretically grounded discipline, it is doubtful that Downs charts the way. Even a skeptical reader, however, will find his novel analysis stimulating and provocative. Although the theme is politics, special attention is devoted to economic applications. As an exploratory study in a central field, the volume should be of interest to social scientists generally.

Downs has a carefully restricted concept of democracy: Politics are the concern chiefly of political parties and individual citizens. A political party is a limited team of office seekers who act only to achieve the power, prestige and income that come with public office. Good deeds are not excluded, but they are only means to these private ends. The members of any one team agree on the deeds (good and bad) that are expedient. The individual citizen has consistent and fixed "political tastes." As far as available information permits, he values the results of different governmental activities in these terms. He votes periodically for one or another party on this basis. Elections are decided by majority rule. All offices go to the victorious party. If there are more than two parties and coalitions, the government is formed by any cooperating group of parties receiving a majority of the total votes.

Under these and other assumptions, Downs analyzes the functioning of democratic politics. A central thesis follows at once: democratic parties seek to achieve only one goal, the maximization of votes. Under alternative assumptions of certainty and uncertainty, further implications are explored regarding the nature, possibilities and consequences of political rationality.

A theory is never fully realistic as to assumptions, and even a very unrealistic one can be suggestive. Downs' analysis nevertheless is properly appraised in terms of assumptions as well as implications. As the author freely acknowledges, his model excludes vital aspects, but as a working hypothesis the basic postulate on party motivation is compared with the corresponding behavioral postulates of economics, from which (as the title of this volume underlines) it is adapted. Adam Smith's famous reasoning about the motives of the butcher, the brewer and the baker supposedly applies "equally well to politics." Therefore, "we accept the self-interest axiom as a cornerstone of our analysis" (p. 28).

One need not consider public officials as a breed apart in order to question this view. After all, social consequences are generally far more important and manifest in public political than in private economic choices. The butcher, the baker and the brewer can less easily afford to consider such consequences than the public official. Failure to *maximize* votes may still leave a party with a victorious majority but failure to maximize profits necessarily involves a financial sacrifice. Regarding income alone, politics are hardly very rewarding; and many persons enter this field only after they have made their fortunes. Perhaps the economist himself is often misguided about motives, if not of

bakers then of corporation executives, but this is no reason to compound his error in politics.

In appraising the remaining assumptions, it should be considered that at least under uncertainty many familiar features, such as persuasive leadership, ideologies, etc., are admitted. The analysis nevertheless still seems to exclude other no less familiar features, such as persuasion regarding ends as distinct from means, "traditional" and "neurotic" voting, falsification, etc.

Among many diverse findings, perhaps most novel is the conclusion that democracy cannot stand certainty. In a two-party system, for example, rationality on the part of an incumbent party calls for it to adopt and to promise to adopt policies on every issue that are favored by a majority of citizens, but unless there is a strong consensus, the opposition may still gain a victory by uniting passionate minorities. If the incumbent party is itself solicitous of such minorities, as can easily be shown, it still may be defeated by appropriate opposition tactics. Moreover, rationality on the part of either party is thwarted to begin with if (as is almost inevitable) there is some one issue on which the majority preferences are inconsistent, *i.e.*, the majority prefers A to B, B to C, and C to A. If (as Downs assumes) voters are guided primarily by past performance, it follows that the incumbents have no incentive to cater to voters' preferences while in office; if promises are what count, elections are decided by the extraneous fact of who commits himself first. These unhappy consequences may still occur but are not inevitable under uncertainty, since all calculations are then made difficult.

For sake of clarity, it may be well to observe first that the two situations which Downs distinguishes where rationality breaks down are in reality essentially one. As may not be stressed sufficiently, what counts ultimately is not the voters' preferences on individual issues but their preferences as between all combinations of all alternative policies on all issues. Passionate minorities cause difficulties, therefore, only if they result in an inconsistency in collective preferences as between alternative combinations of policies of the same sort as occurs if the collective preferences are inconsistent as between alternative policies on one issue. Barring such inconsistency, there is always a program that cannot be beaten. If a tie vote is taken to indicate collective indifference, the reader may readily see how impassioned minorities give rise to inconsistency in collective preferences by working out the majority preferences between alternative policy combinations in an example given by Downs: Table 2, p. 64.

Downs very possibly helps illuminate here a political phenomenon of genuine interest: how passionate minorities make for unstable majorities. One wonders, for example, whether his abstract logic may not have some sort of application to the French parliament. On the other hand, the disconcerting implication that the effective functioning of democracy requires mistaken calculations should be read in the light of the special nature of the model, including the restricted role allowed to persuasive leaders and the assumption about party motivation. If the incumbents have any other than private ends, for example, rationality still is possible (but contrary to what was said

above, given certainty, a benevolent party that fails to maximize votes faces inevitable defeat unless such benevolence is universal, *i.e.*, unscrupulous opponents may take over all of the party's program except for vote-costly benevolent features). Additionally, in Downs' model public office supposedly confers practically unlimited authority. In the real world, the powers of democratic governments are always limited, and partly to areas of relative consensus.

Under certainty, Downs apparently considers that democracy would escape difficulties if there were no impassioned minorities or inconsistent collective preferences on any one issue. Given his theoretic model, however, the incumbents could always formulate an unbeatable program. This might be very discouraging to the opposition.

Inconsistency in collective preferences is deservedly referred to in this study as the "Arrow problem." Although Arrow envisaged the inconsistency as posing a problem of welfare economics, many suspected that it was more pertinent to politics. This study happily makes this fact all the clearer.

The introduction of uncertainty inevitably blurs and complicates the model, and some may wonder whether with this the possible advantages of model building may not be largely lost, but Downs succeeds in illuminating in a striking way the pervasive role of this factor. For students of politics, I suspect one of the more valuable features of the book will be the suggestive elaboration of such aspects, for example, as the relation between uncertainty, party ideologies, and the distribution of power. In order to explain competition between different party ideologies, an ingenious adaptation is made of economic theories of spatial competition.

Under both certainty and uncertainty, government measures affecting resource allocation are examined with special reference to the attainment of a Pareto optimum, *i.e.*, there are no unexploited opportunities for some to gain without others losing. The author treads on familiar ground in stressing the great practical difficulties that are encountered in public action towards this end in respect of collective goods or otherwise. On the other hand, given his general thesis on governmental behavior, the usual arguments regarding the possible adverse effects on economic efficiency of political income redistributions necessarily gain in force. A novel, perhaps useful finding is that the Arrow problem may preclude the continuing realization of a Pareto optimum even if practical difficulties are absent.

Probably of more interest to economists than the discussion of resource allocation will be the thesis that at least in their normative theories, they erroneously assume that democratic governments seek to maximize "social welfare." In reality, it is held, governments consist of men who pursue private ends within the framework of a given political constitution. In prescribing policy, the economist must not assign the government "a social function inconsistent with those motives and that structure, unless he is deliberately advising a change in society's political constitution" (p. 291). Apparently his task rather is to advise the government "how best to please the voters" (p. 292).

Very possibly, economists do tend to assume too easily that public officials are social-minded. It is permissible to doubt nevertheless whether the alternative standpoint proposed in this volume is any more correct. In any case, a question is in order concerning methodology. Telling the government "how best to please the voters" may mean many things, including doing just what normative economics always has been doing. But perhaps understandably some may balk if (as Downs seems to mean) the economist is to take as normative data the private ends of public officials, and a "political constitution" which apparently embraces the momentary distribution of political power. Tariffs may be vote-getters, but this is not a sufficient reason for the economist to advocate them. Downs considers that the normative economist may also counsel individual voters (as distinct from public officials), but has little to say about this approach. On the supposition that public officials are to be counseled in terms of essentially the same values as citizens generally, it has seemed to the reviewer that the counseling of citizens is properly the central task of normative economics.

A recurring theme of this study concerns the need to integrate political and economic theory. For purposes of both positive and normative economics, it is argued, "economists must develop models which unify politics and economics, as we have done in this study" (p. 294). With a fully developed theory of politics, we may agree that the economist might gain in diverse ways from being able to predict political behavior. It does not deprecate Downs' achievement, however, to feel that such eventualities must lie far in the future, if they are ever to be realized at all.

The study concludes with an extensive list of "testable propositions derived from the theory." The list is convenient, but the author perhaps has done himself an injustice by including a good many propositions that clearly fail to be of the "nonobvious" sort sought; for instance: "Democratic governments tend to redistribute income from rich to poor"; "Many citizens who vote and consider voting important are nevertheless not well informed on the issues . . ."; "Democratic governments tend to favor producers more than consumers in their actions."

ABRAM BERGSON

*Harvard University*

*Economic Fictions—A Critique of Subjectivistic Economic Theory.* By PAUL K. CROSSER. (New York: Philosophical Library. 1957. Pp. xxiii, 322. \$4.75.)

For many students of the history of economic reasoning Crosser's book will provide irritating reading. It consists of a series of essays dealing in turn with the "most representative writings" of the "most representative authors of the subjectivistic school of economic thought," namely Thünen, Menger, Wieser, Böhm-Bawerk, J. B. Clark, W. S. Jevons, Schumpeter, Keynes and O. Spann. Crosser has dipped his pen deeply into a bottle filled with philosophical phraseology in order to demonstrate how the teachings of the "classical economists," Adam Smith and David Ricardo have been "deconceptualized, designified,"

transformed into "paradoxes," brought to "meaningless and nothingness" by the marginalist economist, how the teachings of Schumpeter are marked by Intuitionism, those of Keynes by Alogism, those of Spann by Mysticism.

Characteristic of Crosser's attitude towards his problems is a statement in which he identifies the "subjectivistic form of reasoning" with the elimination of the distinction between the imaginary and the nonimaginary, between "fictitiousness and reality," and defines "fiction" as a "postulation which is based on the assumption that a distinction between reality and appearance constitutes an inconceivable proposition."

It might not be amiss to contrast with that problematic definition of "fiction" another which enjoys widespread acceptance and reads that "fiction is a statement known to diverge from the results of our observations and to contain an element admittedly false (See the *Encyclopedia of the Social Sciences*, Vol. VI, p. 227). Thus defined, "fictions" have been a perfectly legitimate, even indispensable instrument of all patterns of thought which are not based on the belief that "reality" can be grasped directly by human reason and that "truth" can be established directly with the aid of appropriate concepts. When Crosser places Newton's famous phrase *Hypotheses non fingo* as a motto at the head of his book, he seems to overlook the fact that Newton's "atom," the indivisible standard unit of the law of gravity, was a fiction; when he extols the logic of the "classicists," Adam Smith and David Ricardo, he ignores the fact that in various passages of their writings the standard unit of labor costs is presented as something like a fiction; he obviously ignores an interesting chapter of H. Vaihinger, *Philosophy of As If* (Transl. 1925) dealing with the role of fictions in Adam Smith's *Wealth of Nations*.

It is obvious that an intensified use of fictions has been made by subsequent authors who realized far more clearly than had Smith or even Ricardo, that the human mind can, at best, arrive at coherent and consistent pictures of reality constructed in accordance with the rules of human reasoning. It is a moot question to what degree the introduction of such fictions into economic doctrine might have led to erroneous interpretations of economic relationships and events, and Crosser would have rendered a great service not only to the history of economic thought, but also to that of Western reasoning if he had carefully analyzed the logical background of the various fictions which have played changing roles in economic doctrines. Instead of undertaking a study of this type, he has considered it his main task to oppose to the "postclassical" developments of economic reasoning the firm belief in some of the main logical principles of Ricardian economics: The conception of value as an outcome of "societal appraisal," the existence of a clear line between "essential" and "nonessential" characteristics, between the "lasting and the passing," the "real" and the "apparent," the distinction between an "intrinsic" nonmonetary value of the goods and an "extrinsic," nonessential monetary value; the distinction between the flow of money and the flow of products (p. 260); the explanation of the elements of the distributive process (wages, rents, profits, interests) in terms of social, not functional relationships and so on. On closer analysis it appears that, what Crosser is fighting for is the preservation of a



medieval heritage: the belief that the substance concept and the consequences drawn from it are indispensable elements of economic analysis.

KARL PRIBRAM

Washington, D.C.

*Economic Issues—A Financial and Economic Debate in the Critical Years 1954-57.* Edited by STEPHEN FROWEN and H. C. HILLMAN. (London: Waterlow and Sons Ltd. 1957. Pp. viii, 231. 21s. 6d.)

The thirty-three essays which compose this volume appeared originally in *The Bankers Magazine* in the years 1954-56. Among their authors are such well-known economists as Roy Harrod, Colin Clark, G. L. S. Shackle, Lawrence R. Klein and R. G. Hawtrey. The individual contributions are short—from five to ten pages each—and are concerned with a wide variety of topics clustered rather loosely about various policy issues which were matters for public discussion and concern in the period covered, such as inflation, monetary controls, budget policy, and problems of convertibility and international economic cooperation. Obviously it is idle to expect either a high degree of coherence or much depth of penetration in such a collection of short pieces culled from the pages of a journal whose principal function is not to advance economic discussion but to service the needs of the banking profession. Indeed, American economists may marvel that in England professional economists should be afforded such an opportunity to discourse directly with practical businessmen through the pages of a trade journal. How the economists responded to this opportunity to present their views—the topics they picked and their manner of approach—may well be of more interest to professional economists outside the United Kingdom than the particular views expressed.

The editors have done their best to provide a modicum of unity by grouping the essays under six general headings. The first group, called "Issues and Views," consists of five assorted review articles and a short essay on inflation. It is not at all clear why the editors chose to reprint the reviews, since none of them is in any way remarkable. The essay on inflation by M. J. Bonn provides a brief description of the relation of budget surpluses and deficits to changes in the money supply and strikes the first few chords in the motif of bank rate which later swells to full orchestra in the debate on monetary policy.

In the second group of essays the authors seek to acquaint their readers with some of the concepts which underlie national income accounting and to illustrate the application of national income statistics and forecasts to policy formation. In an article on "Capital" Colin Clark deprecates the relative neglect of national wealth measures, notes some of the difficulties involved, and endorses a method of valuation for which the rationale is anything but clear since it can result in the assignment of zero capital value to fully maintained and currently used capital equipment.

Possibly because bankers were the intended audience, the essays on the channels and effectiveness of monetary policy are among the more vivid and lively in the collection. Articles by Harrod, Hawtrey and W. T. Newlyn explore questions of the timing of "the credit squeeze," whether it is the cost or



availability of money that is more significant, and the relative importance of commercial bank cash versus liquid-asset holdings as fulcrums for central bank policy. Hawtrey reiterates his view that the primary mechanism through which changes in interest costs affect production and employment is via traders' willingness to hold goods inventories. An essay by Shackle, included under another grouping later in the volume, expounds the familiar view that investment, especially that in fixed plant and equipment, is insensitive to interest changes. One is struck by the absence of fresh ideas and, more important, of the dearth of concrete empirical information which characterizes these exchanges. Surely it is not too much to expect that a thorough-going empirical investigation of the behavior of banks and other lenders as well as borrowers would contribute to the clarification of some of these much-debated issues.

The next two groups of essays are concerned with taxation, fiscal policy and related matters. Among the fresher contributions are three articles by Lawrence Klein utilizing materials drawn from the Oxford Savings Surveys conducted annually since 1952. In one he discusses the implications of savings habits of individuals for budget policy intended to encourage private savings, in another the practicality of estimating personal expenditures (to serve as the base for an expenditure tax) as a residual obtained by subtracting saving from income. The third compares some aspects of the situation of British and American consumers, *e.g.*, sources of income, behavior concerning consumer durables, channels of savings, and so on as revealed in survey research studies in the two countries. In another essay David Walker suggests that inflation reduces the share of corporate profits which goes to the stockholder, since directors can maintain or even increase the dividend rate on issued capital while reducing the percentage of profits paid out.

The final set of essays devoted to international economic relations includes a comment by Harrod on the problem of convertibility as of late 1954, a couple of somewhat technical discussions on the European Payments Union and the General Agreement on Tariffs and Trade, a diagnosis by Hawtrey of the reasons for the weakness of the pound relative to the dollar (too much purchasing power at home directed toward domestic consumption), and an attempt by H. C. Hillman to assess the probable impact on the British economy of an American recession. The last of these gains currency from recent downtrends in the American economy.

There is little of substance in these essays for the professional economist. For the nonprofessional the collection may well lack the appeal of direct relevance to present policy issues. There is also the difficulty that the principle of selecting articles from a single journal inevitably sacrifices something of the quality and balance that could be achieved by drawing on a broader base. One can applaud the original purpose of the essays included in this volume—that of developing communications between economists and an intelligent public—and even admire the grace with which this purpose has been accomplished, while remaining unconvinced of the merit of such a reprint collection.

DONALD R. HODGMAN

*University of California, Berkeley*

*Contributo alla teoria della politica economica.* By FRANCESCO PARRILLO. (Turin: UTET. 1957. Pp. xv, 303. L. 2,400.)

*Politica economica* means "economic policy" as distinguished from *economia politica*, political economy or (as Alfred Marshall taught us to say) economics. Economic policy is not to be confused with action or administration. It is, or should be, a *guide* to action, "A bridge between the oversimplifications of economic analysis and the operational plane [of governmental activity in regard to economic matters]."

Classical and neoclassical economists, says Professor Parrillo (p. 204), tended to view economic life as self-contained and self-directed; the state was regarded as a thing foreign—and by inference harmful—to the body economic. On the whole, therefore, the economic policy most suitable for the state was thought to be one based on *non-agenda* rather than *agenda* (p. 297). Although the dominant role played by government in all modern economic societies—the author might well have added, archaic ones too—has not gone unnoticed by our profession, no consensus has yet been reached on what should be the first principles of an affirmative and scientific economic policy for the guidance of government. This book is an appeal for more attention to this question. It is also an excellent compendium of present and earlier speculations on the scope (but not method) of government's intervention in economic life.

The book's seven chapters are devoted mainly to four topics. Chapters 1 through 4, about a hundred pages in all, afford an overview of the "policy" doctrines expounded by influential 18th and 19th century economists. Chapter 5 provides a hundred-page exposition and critique of more modern Italian thought on the same subject, beginning (as might be expected) with works that appeared during the Mussolini régime—a period which in its impact on Italian economic thinking marks a turning-point as sharp as that which in America ensued from the great depression and the New Deal. Chapter 6 surveys in fifty pages the "theoretical systematization of economic policy by foreign contemporary economists." Included here are not only abstracts of the ideas of the "constructive contributors"—Hicks, Frisch, and a number of others—but also of other people who are to varying degrees skeptical or agnostic in regard to the possibility of finding any universals on which to found a rational public economic policy. Among these may be included Röpke, Myrdal, and of course Lionel Robbins. Finally, Chapter 7 enumerates various goals for economic policy that seem to have wide endorsement. Among these (p. 288) are development of greater productivity; rectifying cyclical fluctuations; broadening markets so as to absorb the added abundance to be afforded by improved techniques; and a better income distribution both spatially and interpersonally.

Parrillo of course perceives that some of these goals may prove mutually inconsistent; a strategy (policy) that will bring us closer to one may by its very adoption interdict us from reaching one or more of the others. He declines to prejudice their relative importance, but says (p. 286) that in science "objectivity can never be disassociated from the power of selectivity and critical judgment"; hence social science is not *prima facie* inadequate to the task of ranking values as well as advising on methods for achieving goals (this

latter being the particular province of economic science as such). Moreover, the author points out quite rightly that the anguish of choice between goals is not necessarily an unavoidable consequence of greater governmental economic intervention, *i.e.*, a strong "economic policy," and such a policy need not have as a built-in feature the greater abridgement of individual liberty. A policy that can greatly expand the parameters of the economic scheme, without unduly delimiting their mutual relationships—leaving these, and the relationships of the variables as well, to determination by market forces—will surely enlarge very greatly every individual's range of choices (p. 217, 298).

*Politica economica* gives all the evidences of excellent scholarship one is accustomed to expect from Italian economists. It would have been, however, more complete had the author made mention of J. K. Galbraith's very interesting view that a suitable component of governmental economic policy is the fostering of countervailing power. The different structure of Italian economic society may explain this omission, which in any case scarcely diminishes the merit of Parrillo's thought-provoking book.

A. STUART HALL

*University of Nebraska*

*The American Economy.* By ALVIN H. HANSEN. (New York: McGraw-Hill. 1957. Pp. xv, 190, \$5.00.)

This book is one of the Economic Handbook Series designed to serve as brief surveys in one-semester courses, as supplementary reading for certain courses and as an aid in the growing field of adult education. The book is in large part a series of six lectures given by the author at the University of Chicago with some supplementary material.

It reviews the economic advances of recent decades, and the gradual transformation of a *laissez-faire* economy to a mixed economy. There is much material on modern techniques for stabilizing and expanding the economy and on the relevance of Keynesian economics to modern problems. The book describes the important advances in economic thinking in recent years as well as those in the areas of stability, growth and distribution—advances which have been facilitated by the development of modern national-income theory.

The author's discussions of monetary policy in an advanced economy and of recent monetary issues are among the most suggestive sections of the book. In a concise analysis of the contemporary problem of inflation in a society committed to a policy of full employment he establishes the generalization that periods of rapid growth have usually been periods of moderate price increases. He then concludes that we probably cannot achieve our real growth potential in the next twenty years without a moderate increase in the price level.

There follows a provocative discussion of the effectiveness of the rate of interest as a means of economic control. This device, he believes, is now out of date because of its adverse effect upon capital values. Since monetary policy can play only a modest role in a stabilization program, primary reliance must be placed upon fiscal policy supplemented when necessary by selective controls of real estate and consumer credit. These general conclusions on monetary policy are buttressed by a discussion of recent monetary issues including

the method of carrying out open-market operations and the relations between the Federal Reserve and Treasury policies.

There is a lengthy section dealing with the Employment Act of 1946 and its history under both Democratic and Republican administrations, together with a summary and evaluation of much of the work of the Council of Economic Advisers and the Joint Economic Committee.

In an impressive chapter dealing with the problem of values in a rich society the author raises questions traditionally not considered a part of the discipline of economics. He suggests that just as Keynes broadened the scope of economics from Marshallian cost accounting and moved it into the realm of political economy, so today economics must develop a concern not merely for full employment and maximum output but also for social priorities, that is, it must become once again a branch of moral philosophy.

The book reflects the mature thinking of one of the outstanding economists of this generation. It will be of real interest not only to the serious adult reader but to the professional economist as well because of its comments on contemporary problems and because of its bold plea for a broadening of horizons within the profession.

JAMES F. CUSICK

*Dartmouth College*

### **Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought**

*Price, Cost and Output.* By P. J. D. WILES. (Oxford: Basil Blackwell. 1956. Pp. xi, 302. \$4.20.)

When *describing* business enterprises, orthodox marginal analysis characteristically assumes that entrepreneurs (1) know what their marginal revenues and marginal costs are, and (2) are motivated to maximize profits by setting prices and outputs indicated by the equation of marginal costs with marginal revenues. When *evaluating* the performance of business enterprises as those institutions by means of which society allocates its resources to meet its wants, welfare economics based on orthodox marginal analysis notes that when demand for the product of the firm is not always perfectly elastic (as, of course, it is not except under competitive conditions) then resources are used in "derogation from consumers' sovereignty and the best allocation of resources." In viewing this orthodox marginal approach to the business enterprise, Wiles notes that (1) the desire to reach welfare conclusions resting on marginal concepts has pushed economists to logical discoveries as to how firms would act if they knew marginal revenues and marginal costs and maximized profits rather than toward factual investigations of actual firm behavior; (2) as a consequence of failing to make factual investigations, economists have inadequate classifications and descriptions of business behavior when marginal revenues and marginal costs sometimes are not known and at other times are not knowable, while at the same time firms do not maximize profits and are subjected to all sorts of pressures ranging from spe-

cific governmental controls to subjective pressures to be "fair"; and (3) the focus on marginal costs leads economists to ignore, or at least not to emphasize, those relatively more important factors which influence long-run average costs, factors such as technological change, improvements in managerial techniques, age of the firm, and the like.

Wiles' views as to the shortcomings of orthodox marginal analysis naturally affect the organization of his book. After introducing the subject and defining terms, Wiles describes the short-run price and output behavior of business enterprises within five descriptive categories. The five categories contain firms of the following different types: (1) "primitive higglers," firms which sell goods not produced by them, or goods for which they do not know costs, to unique customers; (2) "price-takers," firms which find prices set for them in perfect markets; (3) "full-cost producers," firms which are output takers and who set price at average cost plus a chosen margin; (4) "discontinuous producers," firms which produce unique articles, or unique batches of articles, one at a time; and (5) "marginal cost producers," firms which find it necessary to discriminate among customers with respect to price either to avoid losses or to economize on fixed facilities by "evening-out" the peak demand for a perishable product or service. Description of short-run price and output behavior of firms in these five categories points to new or modified meanings for the supply curve and profit maximization. Then follows material on the shape of long-run average cost curves—which are held to be L-shaped, not U-shaped, with a consequent absence of cost limits to plant and/or firm expansion—and determinants of price and output movements in the long run. A final chapter uses the welfare criteria established in Lerner's *Economics of Control* (New York 1944) to evaluate the performance of business enterprises as Wiles describes it.

In detail, not much of Wiles' work will seem new to economists whose interests have pointed them toward an examination of business enterprises and the important niche which they occupy in the economy. To say only this, however, would be grossly unfair to Wiles and to minimize the importance of a first-rate book. The book's importance stems from four characteristics: (1) the fact that it brings together in one book for meticulous examination most of the objections which economists have specifically presented, or inarticulately felt, with respect to the marginalist theory of the firm; (2) the fact that these objections are examined within a framework that assigns each a place in which its relative importance can be assessed; (3) the fact that attempts are made to outline a new general theoretical framework to replace the old where it is found descriptively inadequate; and (4) the fact that the new picture of business performance painted is compared with the welfare ideal (a performance picture which is not, in Wiles' view, as bad as that painted by orthodox marginal analysis). These characteristics would seem to make this book required reading for students who have had a rigorous course in micro-analysis and who wish to look carefully at the business enterprise from either a management or a social point of view.

More specifically, this reviewer found particularly stimulating Wiles' examination of the relationships between accounts (and accounting) and firm

behavior, his balanced discussion of the marginal cost principle, and his admittedly preliminary analysis of the determinants of the price-output equilibrium of the firm in the long run when the long-run cost curve for the firm is L-shaped. On the other hand, this reviewer was struck by the absence of any real consideration of the problems of organization as independent determinants of firm action, an inadequate treatment of the problems of oligopoly, and, in most instances, the replacement of one a priori theoretical system—that of marginal analysis—by another. With respect to the latter point, and except for a statistical study of long-run cost curves, Wiles simply reasons from new assumptions about the nature of the firm and/or its environment to new conclusions about its behavior. True, these new assumptions strike the reviewer as being more “realistic” than the old. True, occasional support for their use is found in the statements, or writings, of entrepreneurs and/or analysts of business behavior. But, at bottom, many basic points are subject to the trap which Wiles so correctly sees as having caught the marginalists. “The indicative mood has a fatal attraction: it is so much easier to say ‘the entrepreneur does’ than ‘if my premises hold the entrepreneur would’” (p. 2).

Finally, this excellent book might well be read with profit along with Howard Bowen's slim volume entitled *The Business Enterprise as a Subject for Research* (New York 1953) written for the Committee on Business Enterprise Research of the Social Science Research Council. Bowen's volume defines “business enterprise research” and the “theory of the firm” and suggests research areas likely to prove fruitful. It is clear that Wiles' book is squarely in the area marked out by Bowen, that he is using techniques congenial to the Bowen report, and that he has at least systematically and meaningfully approached a good many of the research areas suggested by Bowen. He does not have all of the answers, but the economist who hopes to find them might well start with Wiles' work in beginning his search.

CLARK C. BLOOM

*State University of Iowa*

*Income and Wealth, Series VI.* Edited by MILTON GILBERT and RICHARD STONE. (London: Bowes & Bowes. 1957. Pp. xiv. 306. 42s.)

This volume comprises ten of the twenty-three papers originally given at the Fourth Conference of the International Association for Research in Income and Wealth held in the fall of 1956. The papers, which were presumably selected on the ground that they represent contributions of more general interest and permanent value, fall into two parts. Three papers present or evaluate explorations in model building; the remaining papers deal with several important aspects of the measurement and interpretation of size distributions of income.

Carl F. Christ's paper on econometric models of the U. S. economy is a masterpiece of exposition and condensation. In a few pages, the elements and dynamic properties of what he calls Tinbergen-type models are presented. The balance of the brief paper is built around two tables in which the characteristics of nine of the best-known models are summarized and results of ex-



trapolating four of these models for one or two postwar periods are presented. The Klein-Goldberger model, which Christ finds the best, is subjected to additional tests to ascertain its forecasting qualities.

J. Lips and B. D. J. Schouten examine the reliability of the policy model used by the Central Planning Bureau of the Netherlands. P. J. Bjerve explains a model used to forecast bank liquidity in Norway for 1950 and 1951. The model consists essentially of a double-entry table showing estimated changes in intersector claims and debts of the main sectors. Projections were arrived at in different ways: for some cells no change was assumed except for known changes in contractual payments (since there was not sufficient evidence to expect change in either direction), for some others such down-to-earth methods as making guesses on the basis of interviews with currency-control officials were used, and for still others alternative assumptions were made as to results of fiscal operations and as to policy. After following Bjerve through the intricacies of several sets of tables and equations, the reviewer finds himself in a situation somewhat similar to that of Mr. Jurdin in the *Bourgeois Gentilhomme* who suddenly discovered that he was talking prose: he simply did not know that, by having acquired the habit of analyzing the effects of government fiscal and debt operations, of bank lending and investment operations, and of changes in the holding of assets on the liquidity position of the various sectors of the economy, he was building models.

It is possible to demonstrate, as Christ does by comparing the Klein-Goldberger model with a now famous "naive" model of Friedman (which simply assumes no change from the previous year), the superiority of one model over another for a period of years. However, there do not seem to exist any objective criteria for judging the reliability of a model. This reviewer cannot but agree with Lips and Schouten (p. 27) that it is a matter of subjective judgment whether an error of 100 per cent in forecasting year-to-year changes should be regarded as serious or not. Indeed, the permissible error will in each case depend on the use to which results of projections are to be put. Whether the building of economic models will ever yield more than a discipline for the analyst, forcing him to consider a fairly complex set of interrelations within a formalized framework of past experience, is a question which the limited experience with such models is as yet incapable of answering. The three papers in this volume are more likely than not to tilt the balance in favor of the skeptic.

The seven papers on income-size distribution suggest that the interest in this important field, where the multiple and at times contradictory effects of the economic process and of government policies are molded into one, at times deceptively simple set of figures, is gradually shifting from measuring to interpreting. Dorothy S. Brady's paper on "Measurement and Interpretation of the Income Distribution in the United States" draws our attention to the fact that the focus of at least one of the three earliest studies on income distribution in the United States was already on the "why?"; the inquiry into the "what" was meant as a stepping stone for exploring what would happen if the *status quo* was changed.



Mrs. Brady's paper is a rich sampler of problems that have arisen in recent years in trying to draw new economic insight from statistical data on size distribution in the United States. In doing so, she emphasizes questions that are particularly relevant in studying and projecting spending and saving.

Mrs. Brady keenly feels the need for a theoretical framework for analyzing size distributions, and identifies among the results and hypotheses suggested by recent studies some possible building blocks for such a framework. Impressed by the relative symmetry of the distribution of income within specific occupational groups, she explores the factors which may affect departure from symmetry in the upper part of the range; notes the stability of relative incomes within a professional group; and raises, on a more general level, the question of the "transient" and "permanent" elements in income. If her statement that "the statistical elegance of present day estimates of the income distribution in the completeness and consistency of the accounting for the total population and the total income during a given year does not guarantee their usefulness in economic analysis and projection" was intended as a warning to her international audience, which might have been overly impressed by our wealth of our income statistics, this reviewer would not know of any wiser advice to give.

The following three papers are largely descriptive, exploring changes in income distribution in Denmark (K. Bjerke), Canada (S. A. Goldberg and J. B. Podoluk) and Australia (mainly in 1942-43, by H. P. Brown). All three bring together material not available in convenient form elsewhere, explore the peculiarities and shortcomings of the original data, describe steps taken to improve their comparability and consistency, and to a varying extent summarize the main characteristics of the distributions discussed. Bjerke focuses on changes in the distribution of income as well as of wealth in Denmark between 1939 and 1952, and the article devoted to Canada does the same for wages and salaries in the longer interval between 1930-31 and 1951. Brown concludes his paper by suggesting a new measure of inequality which he calls an "inferiority index," and which is related to Gini's  $\delta$ . D. Cole and J. E. G. Utting use data from the 1951 family budget study covering one single county in England (Cambridgeshire) to explore the association between family structure and income. Their explorations on the effect of multiple-earner families on the family size distribution of household incomes, on factors influencing the formation of multiple-earner households, and the life-cycle aspect of household income, even though based on material limited to a narrow geographic area, raise questions of general significance which have as yet been probed into only very superficially in the United States and elsewhere. The remainder of the paper deals with the dependence of size distribution on the unit of measurement (ranging here from individuals to households).

The brief paper by R. Bentzel explores some aspects of the economic interpretation of changes in the inequality of income distribution against the background of the Swedish developments during the last two decades (but without reference to any specific data) and focusing on the effects of changes in the distribution of income on consumption. Using a theoretical model and

assuming plausible values for key parameters he arrives at the conclusion that changes in the structure (as contrasted with the level) of income cannot have had significant effects on the level of consumption.

Noting that "we have a theory of functional distribution running in terms of income shares that can hardly ever be measured, with the consequence that the theory is beyond the reach of empirical verification" (p. 284), O. Aukrist uses Norwegian data since 1930 to investigate cyclical and larger-run changes in the share of wages and salaries in factor income. Aukrist follows Denison's example in narrowing the analysis down to the "ordinary business sector." He finds that export industries account for a greater part of the contracyclical behavior of the ratio of wages to income than home-market industries. Another interesting finding is that interindustrial shifts rather than changes within individual industries account for the bulk of the changes in the wage fraction which, in comparison to 1935-39, has risen in the postwar years for the entire economy, but declined in manufacturing, even though even here interindustry shifts tended to raise this fraction. However, the over-all wage ratio rose because the number of wage earners rose more rapidly than the number of all employed persons, and not because of the relatively greater increase of wages and salaries.

Volumes such as the one reviewed present a mosaic. Their value cannot be judged by whether the mosaic adds up to a picture—it obviously does not—but why should it? The purpose of the conference was to open up new sources of data and to stimulate probing and questioning. Results of such explorations are never lasting: they live on in the stimulus which they give to further research, in the seeds they plant in the minds of those who try to translate administrative data into meaningful economic categories and to answer the question on the essence and implications of change. That there has been change is amply demonstrated by data on size distributions for all countries covered in the present study. Those who reach for this volume will find in it a representative range of technical and analytical problems challenging those working with size distributions of income.

The problems of model building are on a different level: clearly, by using a sufficiently large number of equations a satisfactory model can be constructed to describe almost any web of past relationships. It is the linking of the future with the past that causes all the trouble. Every analyst will have his own specifications in deciding whether a given model is a usable bridge between the two. This reviewer can only repeat Christ's conclusion after reviewing the rich crop of models for the United States: "The reader may make his own appraisal."

GEORGE GARVY

*Federal Reserve Bank of New York*

*L'investissement.* By PIERRE DIETERLEN. (Paris: Librairie Marcel Rivière et Cie. 1957. Pp. 362. 1,200 fr.)

This volume is the sixth in a series "Bilans de la connaissance économique" edited by Robert Mossé, designed to provide a survey of contemporary thought

and of its development in various areas of economics. In French usage, *investissement* corresponds approximately to capital formation, the term *placement* being used for the financial transaction by which investment is facilitated. While *placement* is discussed incidentally, it is to *investissement* proper that Dieterlen addresses himself, devoting successive chapters to definitions, determinants, effects, means, bounds, and policies of investment.

Investment provides a rather unique focus for a survey of economic thought, in that while investment so frequently forms a crucial element of a theory, it is seldom accorded an extensive treatment of its own in the elaboration of that theory. Dieterlen has collected his material from a wide area, ranging from theories of secular growth to monetary theories; the main focus, however, has been with the role of investment in the stability of various macroeconomic systems and models. In fitting material from such diverse sources into his scheme, he has considered his assignment one of catholic coverage and the posing of unresolved questions rather than that of attempting to reduce knowledge in the field to an integrated system. Thus what emerges is a montage rather than a neat picture. But in this way divergent currents of thought are brought together in a striking way, and it may be that the greatest service that this volume will render will lie in the stimulation of further thought by these challenging juxtapositions.

Eclecticism in such brief compass is likely, of course, to become cryptic in spots, as when Kalecki is cited as putting  $\text{profit} = \text{investment} + \text{consumption}$  out of profits, without indicating either the assumptions behind the equation or the precise meaning assigned to the term profit in this context. Also the limitation of the discussion to investment shows rather badly when stabilization is given categorically as the paramount objective of short-term investment policy, without any consideration of the possibility that monetary or especially tax policy might prove sufficiently powerful to take care of stabilization, leaving investment free for the fulfilment of other objectives.

The usefulness of this volume is greatly enhanced by the 80-page annotated bibliography, which abstracts not only the conclusions, but to a considerable extent the essentials of the argument of some 44 books and articles, as they relate to investment. It is perhaps carping to complain that these summaries are in alphabetical rather than chronological or topical order, so that in reading through them sequentially one frequently comes across the criticism of a thesis before the thesis itself, and must work out the relative position of the various pieces for one's self.

This volume should prove even more useful than the preceding ones in the series, in that it covers a field that has more divergence of thought and in which recent developments have been more luxuriant than in other areas of economics. The student will find in it a counterpoint to other more unified but necessarily more restricted treatments of macroeconomics, and even those already well-versed in the field will find it a useful check-list of ideas and points of view that can all too easily be forgotten in the day-to-day work of analysis.

WILLIAM VICKREY

Columbia University

*Nipponkeizai to Keikihendo.* (The Japanese Economy and the Business Cycle.)

Edited by HIDEO AOYAMA. Monograph No. 3. (Tokyo: Kyoto University Research Center of Economic Development and Foreign Trade. 1957. Pp. viii, 264. ¥ 480.)

The symposium under review represents the first attempt of Japanese economists to treat empirically the problem of the business cycle in the concrete context of the Japanese economy. As such, it is a pioneering departure from the traditional and especially postwar preoccupation of Japanese economists with abstract model-building in the field of business cycles. The volume is of special interest to American readers, partly because it reflects the extraordinary influence of the National Bureau approach to business cycles, and partly because it reveals the significant effect of changes in America's propensity to import on the cyclical behavior of the Japanese economy.

The business cycle in the Japanese economy is characterized by two peculiarities, according to the empirical studies made by the contributors to the symposium. First, in terms of *real* national income the Japanese economy has experienced less marked cyclical fluctuations than has the American economy. This statistical fact is interpreted (notably by Hitotsubashi University economists) to mean that the Japanese business cycle expresses itself mainly in price fluctuations and that the Japanese national income deflated by the price index approximates Harrod's "steady growth" without cyclical fluctuations. The Kyoto University economists participating in the symposium, on the other hand, seem to feel that Japan's "aggregate economic activity" in the National Bureau sense (of including financial and trade indices) and in real terms definitely exhibits "cyclical growth" of the Schumpeter-Goodwin variety. Both interpretations seem to neglect the historical role of the government sector in reducing the structural instability of the Japanese economy via an increasing combination of paternalistic subsidies to private enterprise, the nationalization of public utilities and other select industries, and the provision of welfare-statist "built-in" stabilizers. Both interpretations are sidetracked by the statistical issues of "real" vs. "monetary" indices and "aggregate economic activity" vs. "gross national income" estimates.

Second, the symposium reveals that the cyclical behavior of the Japanese economy is covariant with that of the world's effective demand. Noteworthy, in this regard, is the finding that the ratio of the Japanese activity index to the American activity index has covaried with the index of Japan's trade balance with the United States, thus indicating the extent to which Japan's domestic prosperity and depression are affected by fluctuations in the United States. Japan is not alone in entertaining misgivings about the cyclical instability of the American economy, if one may judge from the apprehensions expressed by the economists of other trading nations in *The Business Cycle in the Post-War World*, E. Lundberg, ed. (New York 1955). The symposium shows that the vulnerability of the Japanese economy to the dictates of international economic fluctuation has increased considerably since the end of the second world war. Japanese economists, therefore, are understandably interested in the quantitative studies of the international income-price elasticities of demand for imports, comparative marginal propensities to import,

and the international balances of payments. It is a pity that the symposium does not discuss the relation between foreign trade and economic development as well.

Valuable though the above findings are, the volume under review suffers from the following weaknesses. First, there are too many mechanical applications of the National Bureau technique to the Japanese business cycle without a critical examination of the *conceptual* (in contradistinction to statistical) issues involved in "reference cycles," "reference dates," "aggregate economic activity," "leads and lags," and "peaks and troughs." Second, the familiar criticisms of the National Bureau approach (notably by Koopmans, Hansen, and Metzler) remain unanswered, with the possible result that the contributors to the symposium could be misunderstood as preferring "the jungle of facts and figures" to "the dreamland of equilibrium." The Mitchell-Burns argument that the range of fact-finding should not be restricted by any particular hypothesis is not posited with the counterargument that bold and imaginative hypotheses are impossible to make so long as economists are narrowly confined to facts and figures. Lastly, there is in the symposium an unwarranted aversion to the theoretical approach to the business cycle, for after all the synthesis of theoretical hypotheses and empirical investigations is the *raison d'être* of econometrics. In sum, fact-finding must be coupled with hypothesis-testing and parameter-fitting if business-cycle research is to be fruitful for both analysis and policy.

KENNETH K. KURIHARA

Rutgers University

*Gleichgewicht und Konjunkturtheorie.* By KLAUS ERICH RHODE. (Stuttgart: Gustav Fischer. 1957. Pp. xii, 236. DM 18.00.)

The relationship between equilibrium analysis and business-cycle theory has been a controversial issue in German theoretical literature. The discussion began in 1926 with the significant contribution of A. Lowe with regard to whether a systematic theory of the cycle is possible. In the debate, Spiethoff rejected any form of equilibrium analysis as incompatible with cycle theory. F. A. Lutz denied the possibility of recurrent cycles as well as of a systematic business-cycle theory. Subsequent work built largely upon Lowe's thesis that a theory of the cycle is possible only if statics is replaced by dynamics.

Accepting Frisch's definition of dynamic equilibrium, the author seeks to discover the significance of the evolution from statics to dynamics for our understanding of the cycle. In addition, he compares the typical factual pattern with the various theories of the cycle to ascertain which theories are compatible with the typical Juglar cycle.

The aim of the first task is to replace the present "pluralistic meanings" of the equilibrium concept by an integrated typology of equilibrium theories. This typology should exhibit three characteristics: There should be a logical continuity of the equilibria from the individual units to the whole economy. Continuity should be followed by a declining degree of abstraction in the sequence of models. Finally, dynamic equilibrium theory should provide the "analytical instruments necessary for explaining the cycle."

Logical continuity calls for overcoming the dichotomy between micro- and macro-analysis. Required is a microeconomic total model, which starts from the plans of individual units, proceeds to the equilibria in markets, and ends with a total equilibrium for the economy. Such a model is preferable because it is built upon the motives of the individual units. Yet the micro-total model pertains only to one moment of time. A change of the magnitudes over time is required for an explanation of the business cycle. The resultant difficulties are not resolved and the effort to replace macro-theory is given up.

A distinction between "abstract" and "concrete" equilibrium theories is introduced. Neither term is defined. We find short summaries of the identity of saving and investment, of the acceleration principle, and of so-called "core processes." A random comparison of the strategic relations in different theories is supposed to show, via a diminishing degree of abstraction, "the meaningful connection between the various forms of the idea of equilibrium and the principle instruments of business cycle analysis" (p. 114).

A similar discernment prevails in the discussion of static and dynamic models. The thesis that cycle theory is not possible within the field of static theory is strongly underlined. Yet it is not clear whether the static model is one of full or less-than-full employment. Nor is the relationship between cyclical and developmental models examined. In refraining from analyzing substantive problems, the author deprived himself of the opportunity to achieve a "synthesis of the types of equilibrium analyses."

The comparison between the factual pattern and theories of the cycle is handled with much more understanding. Four questions are considered: How do selected theories explain: (a) the causes of the "first" upswing, (b) why the upswing is a cumulative process, (c) why there must be a downswing, and (d) why cycles recur? The theories of Wicksell, Schumpeter and Spiethoff as well as of Halm, Haberler and Jöhr, are examined for their compatibility with the typical factual cycle. Keynesian theory is not included. Examination of the theories mentioned leads the author to two conclusions: He accepts Spiethoff's position that even the "first" upswing should not be deduced from an equilibrium position of full employment. Such a static beginning forces us to disregard the idle resources of the depression as a cause for the upswing, and permits us only to examine the growth element (known as overproduction) of the first cycle. Yet the author insists that Spiethoff was wrong when he asserted an inescapable conflict between equilibrium analysis and cycle theory. Both become compatible when—following Schumpeter—the upswing is explained out of the disequilibrium of the depression. Compatibility is achieved because the equilibrium point is then located in the middle of the cycle, deviations above or below constituting situations of over- or underemployment.

Yet this oscillation theory does not consider two other objections of Spiethoff to dynamic cycle theory. His objections deal with the impossibility of transforming disequilibria into a cumulative process, and of deriving numerical from equilibrium values. In not examining the oscillations substantively, the author cannot disprove Spiethoff's position on the cumulative process. Yet there is an indirect effort to disprove Spiethoff's assertion on equilibrium



values. Schumpeter's characterization of equilibrium as a "point of reference" is translated into German as a *Massstab*, which standard presumably enables us to measure the distance between equilibrium and disequilibrium points. Of course, Schumpeter's figure of speech did not imply numerical values. The author's thesis that Spiethoff's critique did apply to only static and not to dynamic theory, and that the latter is fully compatible with Spiethoff's own theory, is not convincing to this reviewer.

ARTHUR SCHWEITZER

*Indiana University*

*Veblenism, A New Critique.* By LEV E. DOBRIANSKY. (Washington: Public Affairs Press. 1957. Pp. xii, 409. \$6.00.)

Professor Lev E. Dobriansky of Georgetown University administers to Veblen the same type of medicine that Veblen used on his victims—to lay bare their fundamental philosophical preconceptions. In pursuit of this task Dobriansky reveals his own preconceptions. He has written a critique of Veblen in terms of the metaphysics of *Philosophia Perennis*, a body of philosophical doctrine compounded during the middle ages from the ideas of many doctors among whom St. Augustine and St. Thomas were the leading architects. *Philosophia Perennis* is characterized as striking a happy balance between operationalism and essentialism, becoming and being, synthesis and analysis, empiricism and rationalism, abstraction and concreteness, theory and practice, dynamics and statics, organicism and individualism.

Within the framework of his philosophical approach, Dobriansky sets three objectives: (1) to present Veblen's ideas as a systematic body of thought; (2) to make a contribution to the integration of the social sciences with the aid of systematic Veblenism; and (3) to shed light on the practical alternative to "totalitarian-bound socialism," on the one hand, and "an abstractionist reversion to the socially disintegrative tendencies of laissez-faire capitalism," on the other hand. The first objective is more fully realized than the other two. After a brief survey of Veblen the man, Dobriansky takes up in successive chapters medieval philosophy, modern philosophy, Veblen's concept of science, his evolutionary economic science, his cultural analysis, institutional economic theory, and Veblenian managerialism.

Dobriansky is a trained philosopher as well as an economist, and his chapters on medieval and modern philosophy are likely to overwhelm the unphilosophical economist. Relatively few who read these chapters with understanding will accept the author's view that modern philosophy from Descartes to Dewey has been a frightful and unnecessary muddle which could have been avoided if *Philosophia Perennis* had not been ignored or rejected. The author does not face up to the reality that most modern philosophers, scientists and economists reject the metaphysical absolutes of medieval philosophy. Whatever the intrinsic merits of *Philosophia Perennis*, Dobriansky's two chapters on philosophy, constituting approximately one-fourth of the entire volume, may well discourage most economists from venturing further into the book.



Veblen's philosophical position is labeled "radical empiricism." According to Dobriansky, this antirationalistic epistemology caused Veblen to over-emphasize process and becoming to the neglect of essence and being. Veblen's evolutionary science is said to assume naively that discovery of the origins of institutions is a substitute for an analysis of their nature (being). In order to surmount some of the obvious shortcomings of a one-sided empiricism, we are told that Veblen surreptitiously slips in some rationalistic elements. One is his use of instincts, which however turn out to be moralistic precepts in the guise of scientific categories. While Veblen paid lip-service to the unity of science, his defective method precluded genuine progress toward that goal.

Although Dobriansky thinks Veblen's economics leaves much to be desired, he rates it as a landmark in the history of economic thought. Veblen's cultural analysis and institutional theory represent significant contributions to what economics should be. Dobriansky's ideal appears to be some sort of combination of a Veblen-type evolutionary economics and Lionel Robbins means-end principles. In place of "economics" or "political economy," Dobriansky prefers the term "social economy," which he defines as "a philosophical science which analyzes and interprets institutionalized human behavior as a relationship between ends and scarce means which have alternative uses" (p. 215). Veblen's holistic conception of society, organically related in all its parts, yields valuable insights into the conditions of the good society, which the author refers to in passing as "an organically integrated pluralistic society" (p. ix).

On policy issues Dobriansky contends Veblen was unreceptive to communism, socialism, and New Deal-type interventionism. He asserts that nationalization was for Veblen just another form of absentee ownership. Since Veblen viewed government as a weapon of the vested interests, he mistrusted it as guardian of the rights of the common man. Veblen's program is described as technocratic managerialism under political anarchy: a blend of James Burnham's managerialism, technocracy, and guild socialism. Veblen's program is characterized as utopian; any attempt to adopt his managerialism would spell disaster for democratic institutions and also fail to realize the material abundance which Veblen's engineers were supposed to bring to the underlying population after elimination of the price system. While Dobriansky's interpretations of Veblen's views on policy merit careful consideration, they are by no means simple deductions from Veblen's writings, because on most of these issues Veblen is not clear. Other interpretations are possible.

Despite a penchant for polysyllabic words, complex sentences, and philosophical jargon, Dobriansky has written a significant and scholarly book. Within the framework of his own metaphysical preconceptions and in relation to the goal he sets for himself, he achieves a high degree of success. One may wish that he had employed a different framework, but such a judgment must be conditioned by the reminder that in metaphysics everyone is entitled to his own illusions.

DUDLEY DILLARD

*University of Maryland*

**Economic History; Economic Development; National Economies**

*Economic Analysis and Policy in Underdeveloped Countries.* By P. T. BAUER. (Durham: Duke University Press. London: Cambridge University Press. 1957. Pp. xviii, 145. \$3.00.)

This volume contains a series of lectures delivered by the author at the Commonwealth-Studies Center of Duke University. The first lecture deals with the scope, method and limitations of economics in the study of underdeveloped countries, and the effects of the increased interest in underdeveloped economies on the development of economic theory and policy. The second lecture considers features and developments characteristic of underdeveloped economies, while the third examines problems of policy relevant to a large number of underdeveloped countries. These studies raise few new issues; their chief contribution lies in drawing attention to matters which have been handled inadequately in the literature and practice relating to underdeveloped economies.

However, in some cases the author comes close to a kind of extreme opposite to that against which he warns. For instance, he relies too heavily on direct observation: while the occupational statistics show trade to be a relatively "insignificant occupation" in West Africa, Bauer observed a "large volume of trading activity" absorbing "a large volume of resources," and was thereby led "to doubt the empirical and analytical bases of some widely accepted propositions about the relation between economic progress and occupational distribution" (pp. 13-14, 67-69). But quantification, statistics and, mainly, economic principles provide an indispensable check to direct observation. Compartmentalization, and limited specialization and trade are fundamental characteristics of underdeveloped economies, noted by Bauer in other contexts (pp. xii, 26, 50, 58, 61-62, 69). In addition, production in agriculture and industry requires a relatively large volume of resources, partly because it is conducted on a small scale (see p. 67). Hence, proportionately fewer resources are employed in trade than in more advanced countries. It is unfortunate that Bauer allowed direct observation to override these basic propositions (pp. 13-14, 26-27, 67-9).

In examining Nurkse's international demonstration effect Bauer overemphasizes advantageous imitation; he concludes that contact with more advanced countries "almost invariably accelerates economic growth" and states that "this indeed is a commonplace of economic history" (pp. 26, 65). But the tendency to imitate is a basic determinant of all kinds of economic, political and social behavior: for instance, it is not unusual for workers in underdeveloped countries to bargain for wages and other benefits comparable to those in advanced countries in spite of their much lower productivity, and for governments, public servants and capital-owners to "adopt technical, educational and social standards which are inappropriate and wasteful" (p. 66). It is not immediately obvious, nor has economic history or direct observation proved, that international demonstration may not, in the net, have impeded the economic development of some countries, just as it may have

promoted the development of others. Again, it does not seem "obviously inconsistent" to believe, as the "distinguished Indian" criticized by Bauer does, that, while "the great majority of Indians . . . [have] a strong leaning towards asceticism," "higher incomes would attract luxury or semi-luxury imports" and lead to "the construction of cinemas, the manufacture of soft drinks," and similar uses of capital (p. 17).

The damage from direct observation which is not complete (much of it can not be) or which is extrapolated unduly in forecasting future developments tends to have multiplier effects: since such observations and conclusions are presented vividly and come "from the horse's mouth" they may be accepted readily by less critical scholars even though they clash with established principles of economics and logic.

In other parts of his analysis Bauer seems to neglect factors which are fundamental in the discussion of economic development, for instance, the significance and impact of different rates of change in the study of response to economic incentives. Yet consumption patterns, saving, business behavior and economic welfare in many underdeveloped countries may be different from those in the West because the former countries are stagnant rather than underdeveloped, or because the rates and patterns of change in incomes, leisure or prices are different. For instance, the rate of change, rather than the level, of wages may help in explaining cases of a backward-rising supply curve of work; restrictive economic measures are probably just as much a function of the rate and pattern of economic development as they are of the degree of specialization of the economy (p. 77); and many underdeveloped countries may have a low saving-income ratio because they are stagnant rather than poor. The last proposition rather than the "substantial capital formation in agriculture," would solve the riddle: "it is difficult to see how the developed countries could have reached their present position since all developed countries began by being underdeveloped" (p. 63). The distinction between underdevelopment and stagnation and the features and problems relating to the two conditions has been neglected badly in most of the literature on underdeveloped economies; yet if such countries advance rapidly they may for a time share the former features while shedding the latter.

S. G. TRIANTIS

*University of Toronto*

*Management of Direct Investments in Less Developed Countries.* A Report submitted to the International Bank for Reconstruction and Development. By the Foundation for Economic Research of the University of Amsterdam. (Leiden: H. E. Stenfert Kroese. 1957. Pp. 238. f 16.50.)

The title of this work may be somewhat misleading. Readers may well expect it to be a study of the experience of management of direct foreign investments in underdeveloped countries, based on data obtained in a number of representative countries in this category, sufficient to give it universal value. This seems, implicitly, to be claimed for it. As a report to the International Bank it does not offer practical conclusions on which loan policy

may be based, nor is it discernibly theoretical, although theories of Frank H. Knight and Joseph A. Schumpeter were chosen to constitute its nonpractical framework. It is mainly of an advisory character.

The study is limited to Dutch, French and Belgian foreign investments in underdeveloped countries and to foreign enterprises in the Netherlands. J. F. Haccoû, economist in the University of Amsterdam, who is the author of the work, rejects cultural, sociological and economic criteria for determining stages of development comparatively. In a sustained discussion of prerequisites, characteristics and conditions, which is the main part of the book, he examines aspects of such investments which are in general common to the areas studied, although with important differences of degree.

For the purposes of this report conduciveness to successful foreign investment is made the essential criterion for development. But this makes the effort to eliminate the cultural and economic criteria hardly worth while, for prerequisites that may be essential for, and conditions that may be conducive to, development cannot be satisfactorily appraised without reference to culture, duly defined, and to economic conditions.

The aspect which has received most emphasis is "homogeneity" of purpose of all managerial personnel of the metropolitan entrepreneur. The purpose is, of course, profit. The prospective gain must be large enough to warrant incurring every conceivable risk. So calculability and virtual certainty of return are essential conditions for combination of factors into a "dynamic entity" for maximum productivity, while profit must be maximized. Haccoû asks for complete cooperation, to this end, in the countries where the investments are made. He differs from the relevant part of Knight's theory which he evidently chose as a basis for the discussion of profit. While Knight makes profit or loss depend on the "rashness or timidity of entrepreneurs as a class" rather than on "abundance or scarcity of mere ability to manage business successfully,"<sup>1</sup> Haccoû discusses profits as the result of managerial functions. His concern about minimum risk and maximum profit seems undue. The entrepreneur cannot assume that profit and risk are mutually dependent, but it is common business experience that rates of return vary directly, not inversely, with risk.

The location of the top-management of a firm investing in an underdeveloped country, elsewhere than in that country, was found to be frequently the case in all areas studied, but we cannot conclude, from the evidence offered, that production costs are usually significantly higher because of separation; and it is difficult to accept Haccoû's generalization that spatial separation of the entrepreneur from plant management will cause resentment in the underdeveloped country. Absentee ownership is resented in many underdeveloped countries; but the actual presence of entrepreneurs in such countries could not preclude this result. Usually the objections are two: (1) the low rates of payment for field and factory work, and (2) the unlikelihood that a sub-

<sup>1</sup> See *Risk, Uncertainty and Profit* (Boston 1921, reprinted by the London School of Economics and Political Science, University of London, 1946), p. 283.

stantial part of dividends and undistributed profits will be reinvested in the underdeveloped country.

At the core of the book Haccoû makes direct foreign investment on a "Western large scale" depend on technical possibilities in the country. He conceives of development as typically starting with an economy dominated by import activities, and he allows for the possibility of expansion of investment as technical preparedness increases. But his "dynamic entities" are to achieve stability, presumably in the short run, and thereafter to remain static for long periods during which the foreign investors are to enjoy calculability of returns. This would preclude sustained speedy development in which structures for production must change.

It is admitted that governments of underdeveloped countries have a role as investors, but investment in basic assets such as roads and harbors is preferred for them, as prerequisites for private investments, and it is recommended that they guarantee any of these investments which are made by private enterprise.

New or increased direct foreign investments and foreign initiative would undoubtedly contribute largely to economic growth in underdeveloped countries, but it does not appear that many of these countries are in a position to provide a substantial part of the capital needed, from taxes and tariffs, for construction of basic assets, as Haccoû suggests, or that direct foreign investment and initiative would suffice, even if the governments did substantially increase expenditures for these purposes out of current revenues.

The recent recommendations made by the United Nations' experts, the Millikan-Rostow proposal, and the scale of the Soviet Union's aid for these purposes seem to be evidence enough of the magnitude of the capital investments necessary for basic equipment.<sup>2</sup> The United Nations' experts' recommendation of \$10 billion per annum is a minimum, and the report does not show that needs of capital in the Belgian Congo, French Equatorial Africa or Indonesia, as a part of total needs in underdeveloped countries, are proportionately less.

A narrow sense of nationalism in countries sensitively conscious of newly won independence is repeatedly mentioned as a deterrent to foreign investments. This is undoubtedly true in some cases. Nevertheless cognizance has to be taken of the onerous task of urgently needed development in former colonies. Haccoû's belief in inevitability of extreme gradualness of economic growth in these countries seems too pessimistic. The progress of projects of the TVA type in India does not support this conclusion.

<sup>2</sup> The United Nations' experts recommended additional annual capital of \$10 billion from industrial countries, for a 2 per cent increase in per capita income in underdeveloped countries. See Gunnar Myrdal, *An International Economy* (New York 1956), p. 123. The Rostow-Millikan proposal was that Congress supply \$10-\$12 billion in capital and assistance. And John Foster Dulles testified to the Foreign Relations Committee on the Mutual Security Act of 1956 that the Soviet Union's economic and technical assistance to free countries was approximately \$600 million in 1956. See The American Assembly, *International Stability and Progress* (Graduate School of Business, Columbia University, May 1957), pp. 86, 91.

The colonial power usually leaves a very inadequate basic structure, since the desire for complementarity rather than for autonomous development guided its policy. The greater the deficiency the greater must be the responsibility of the new government for displacement of the "equilibrium state previously existing,"<sup>2</sup> and the more urgent the need of increase in the national income. Consequently, the greater must be this government's concern about disturbances caused by foreign investors whose plans could distort its own design for growth.

WILLIAM E. GORDON

Marquette University

<sup>2</sup> J. A. Schumpeter, *The Theory of Economic Development* (Cambridge 1951), p. 64.

*Contribuições à análise do desenvolvimento econômico.* (Rio de Janeiro: Liv. Agir. 1957. Pp. 354. Cr\$ 300,00.)

This is a collection of essays, written in homage to Professor Eugenio Gudín, by a great variety of authors, some American, some English, some French, and some Brazilian. Those who might shy away because the title is in Portuguese should be reassured; the essays are in the usual tongues of the writers, so that about a third of the book is in English, about a third in French, and about a third in Portuguese. The non-Brazilian authors are drawn mostly from those fortunate souls who have had the privilege of visiting Brazil to lecture or for some other good purpose. Seldom, I think, can a *Festschrift* have been written with such uniform feelings of warmth and affection for its guest of honor; Gudín is regarded with deep respect and affection by all who know him, and while one cannot begrudge his country the long and valuable service which he has given it, one regrets also the linguistic barrier which to some extent isolates Brazil and prevents her great men from receiving the world attention which they often deserve.

The articles reflect in a curious degree the present condition and interests of economics in the three main national groups from which the authors are drawn. The Americans mainly write what might be called notes on applied theoretical economics; the theoretical frame of reference is pretty much taken for granted, and the interesting problems are seen to be those of its application, especially, of course, to countries like Brazil. Thus Bernstein writes on tariff protection and economic development, Brozen on industrialization, Arthur E. Burns on some limitations of economic assistance, Haberler and this reviewer on inflation, Norkse on the problem of fluctuations in the export of primary products, Singer on gains from trade, Viner on disguised unemployment. The French authors—notably Leon Buquet, Maurice Byé and Daniel Villey, write long, often rather elementary, theoretical articles, as if they were still in the middle of the theoretical excitement which prevailed in English-speaking economics a generation ago, as no doubt they are. The Brazilians combine both interests: Bulhões on external economics, Furtada on the marginal analysis and Sampaio on the equation of exchange are on the theoretical, or French pattern, Kafka, on the capacity of absorption of foreign capital, Borges and Loeb and Martins on problems of the labor



force, and Roderigues on Brazilian fiscal policy, are on the applied, or American pattern. The lone Englishman, Lionel Robbins, contributes a Note on the Formal Content of the Traditional Theory of International Trade.

One does not expect high uniform quality from the pieces in a *Festschrift*. The present volume however has a unity and a sustained interest which is rare in such collections. And it reflects in an interesting manner the present position of Brazil not only as a country with an economic history and problems of unusual interest, but also as a place where an attempt is being made to synthesize the Anglo-Saxon and the French contributions to life and thought.

KENNETH E. BOULDING

University of Michigan

*Preliminary Report—December 1956.* By Royal Commission on Canada's Economic Prospects. (Ottawa: E. Cloutier. 1956. Pp. 142.)

"The Gordon Report," whose preliminary draft we here review, constitutes one of the most ambitious efforts at political self-assessment and economic forecasting in economic history. The initial volume has been followed by a dozen or so subordinate ones, and others are in progress. At the end the Commission will undertake a final reassessment. We will not attempt at this time to treat the various specific subordinate reports nor the statistical forecasts contained in the preliminary one. All show a high degree of technical skill and responsibility. Doubtless there may be flaws on the technical side—for who is infallible? But all of the issues to date bear the unmistakable marks of care and competence. The problems which concern me lie on quite another level.

Putting the matter in a nutshell, all this careful technical work tends to take more or less for granted one enormous *ceteris paribus* assumption. That assumption is that the institutional and cultural organization of Canada will continue to show the same dynamism in the future that it has shown in the past. But since the report itself has certain implications or undertones which, in my opinion, seriously call in question this very point, it does not seem out of place to consider in this review some of these cultural-institutional problems.

My task in reviewing, as an American teaching in Canada, has been made easier by the previous review in the *Queen's Quarterly* by Jacob Viner, a native Canadian teaching in the United States. But I find myself a bit more sympathetic than Viner. For though I was born at the other end of the North American continent the problems of the U. S. South and those of Canada have a recognizable resemblance. There is the same desire for economic growth coupled with the same fear of cultural domination. It seems to me quite natural that Canadians should want to see the great part of their wealth controlled by Canadian hands. The question is how high a price are they willing to pay? And what are the best means for ensuring the desired relative independence?

Let us first deal with a subordinate point. Since Canada is relatively a wealthy nation, it seems also reasonable to me for Canadians to want to encourage a diversified, light, consumer-goods industrial development at home.



Primary products are notoriously unstable; and it is not unreasonable to take moderate means for making the home market somewhat less dependent. Canada is growing and the infant industry argument is particularly applicable. But with virtually all economic theory behind me, I submit that it would be far cheaper and more desirable to do the job by subsidies than by tariffs. Here the preliminary report offers (p. 68) a meritorious suggestion. Let light industry which it is desired to sponsor, have a quicker rate of write-off on its plant. Personally, it seems to me, still more direct subsidy might be permissible.

But we come now to the more fundamental question—that of foreign capital investment in Canada and foreign control. The basic problem here is that people who risk their money in new investment naturally want to control the way that money is spent. Without control they are unwilling to take risks. On the other hand the receiving nation wants the investment but resents the control. All these tensions can be found stretching through United States history from Jamestown 1607 to 1914. During that entire period the American colonies and the later nation were net debtors to Europe on capital account. The preliminary report here distinguishes between the U. S. experience and Canada's by saying that foreign investment in the United States was in debts not equities. I am not sufficiently a historian to check this, but I have an idea research might show the distinction considerably overdrawn.

What really interests and disquiets me, however, are certain singular omissions in the remedies suggested by the report. Granted the need, in general, of more capital in Canada, would it not be advisable instead of grumbling so much about the Americans and other foreigners to try to build up the Canadians? To be more specific, why not change the Canadian tax laws to foster a higher average rate of *Canadian* saving and capital accumulation? Putting it bluntly I would say that a capital-hungry country like Canada ought to have a considerably less steep rate of income tax progression and also of inheritance taxation. The (so far) brilliantly successful West German income tax rates might be in point here. But the preliminary report says nothing of any such idea.

Again if it is desired to have more Canadian-managed and -owned businesses and more Canadian executives in foreign-owned business, why not do something to increase the extent and level of Canadian business education? The report does not mention this idea either.

Still more extraordinary is the manner in which the report, while questioning the extent of foreign investment control, goes out of its way to exonerate (p. 93) foreign union influence. I should have thought that the effects of a wage policy more interested in protecting entrenched foreign wage groups than in developing Canada, would have been at the very least, to put it mildly, quite as distasteful to any Canadian interested in his country's development. But, indeed, the preliminary report seems to me to have a certain (albeit doubtless unconscious) labor-controllist bias. The "soft" policy toward cartels, perhaps adumbrated on page 69, may be a case in point; though to be sure the railroad unions do receive a suave chiding on page 78.

I feel sure that an honest expression of opinion, however strange, will not be resented by those who so strongly believe in tolerance and free speech.

And so I am obliged to submit that the greatest danger I see to Canadian growth and the realization of all the glowing forecasts contained in this report is not U. S. investment penetration, but the ideological or cultural penetration of that European and English left-wing labor-socialist philosophy which has shown itself so decisively inferior in the matter of economic growth. Suppose Canada increasingly penalizes Canadian capital accumulation and income incentive, permits more and more irresponsible clamor for money-wage increases, regardless of productivity or employment, sees an increased fostering of the quack idea that economic growth and job security are capable of 100 per cent reconciliation, finds its youth increasingly taught that the desire for achievement and a career are neurotic, silly and immoral, and, on the other side, allows its capitalism to cease to be an enterprise one of *independent* policy and helps it become a mere cosy coalition of vested interests under a "mixed economy" slogan—suppose all these things, the inevitable fruits of the European left outlook, what then will happen to the rosy predictions of the Gordon Report?

Such, according to my misguided brain, are the real dangers to Canadian (and Canadian-owned) expansion. And it seems to me not impossible that the fear of "foreign investment" could turn out to have been the most effective means of ending not just foreign enterprise but also that vigorous home enterprise from which Canada in the past has so signally benefited.

DAVID McCORD WRIGHT

McGill University

*The Chinese Economy.* By SOLOMON ADLER. (New York: Monthly Review Press. 1957. Pp. xi, 276. \$5.00.)

To say that the volume under review is pro-Communist and anti-Nationalist is not entirely accurate. While condemning the previous regime at every opportunity, it goes to such length in praising and justifying every step or policy of the new regime that it sounds as if Peiping speaks. In an important sense, that is true, for the author relies almost entirely on English-language materials officially released by the Chinese Communist authorities, and accepts them without question. Furthermore, the volume is essentially a restatement of the first five-year plan, with enthusiastic running comments and with a review of progress up to the middle of 1956. Therefore, it stands far apart, for example, from W. W. Rostow's *Prospects for Communist China* (New York 1954), which, though outdated in its quantitative data, shows proper restraint and balanced judgment regarding the historical development. With a volume of this nature, it would be futile to argue about matters of interpretation. This review will be confined to comments on the data used.

The first fifty pages are introductory, dealing with the pre-1949 background, progress up to 1952, and the sectorial composition of the economy. The rest of the volume is devoted to the first five-year plan, following very much the same order of treatment, with two exceptions, as in the original Chinese version—planning, industry, agriculture, transportation and commerce, and living conditions. The exceptions are one chapter on finance and another on foreign trade, topics that are not treated as such in the plan.

The statistical data used are chiefly derived from the annual communiqués of the State Statistical Bureau and the reports of high government officials—documents available in English. Although noting that the figures are not always reconcilable “perhaps because the coverage is not identical and because of revisions of preliminary data for the earlier years” (p. 81n), the author does not fully appreciate the significance of this fact. If the data for the earlier years are revised, it is important to make use of only the revised figures. The State Statistical Bureau, which was established in late 1952, took more than a year to set up something like a national statistical reporting system. Since then, the quality and coverage of statistical work has clearly improved and an effort has been made to correct (practically always upwards) some of the data for the earlier years, particularly for 1952 through 1954. In view of the lack of a national reporting apparatus before 1954, it is questionable if sufficient revision of the figures for 1952 and especially for 1949 can be made so as to make them comparable with those of the later years. That being the case, what conclusions can legitimately be drawn by comparing the figures, say, for 1955 with those of 1949? As is well known, the statistical work in the 1930's was very inadequate. What meaning then may be given to conclusions based on comparison of data for 1952 or 1955 with those for the prewar years, as the author repeatedly does in this volume?

Using data available only in English has serious limitations. A great deal of quantitative and qualitative information is found in the periodical literature published in Chinese, such as newspapers, magazines and academic journals. The results of national surveys have been released from time to time. Failure to get access to these sources, plus uncritical acceptance of what is available to him, has led the author to many erroneous observations of fact. One example will suffice. In the chapter on finance, a field in which the author professionally has been an expert, it is stated that the note issue has not been used to finance budgetary deficits and that even in 1950 (the year the Korean war started) bond sales had more than covered the gap between current revenue and current expenditure. As a matter of fact, a gap of 289 million yuan had to be covered by note issue in that year—and the information was published in January 1955. The author stresses the soundness of Communist China's national finances by referring to the large annual budgetary “carry-over” up through 1955. But as repeatedly pointed out by the Department of Budget in Peiping since the middle of 1955, the so-called surplus had invariably been *currently* used for credit expansion by the People's Bank. Budgetary spending of the carry-overs would therefore result inevitably in an increase in note issue, as the realized budget for 1956 testified. Only an uncritical acceptance of the official data without an attempt to inquire into their meaning from Chinese sources has led the author to reach the unwarranted conclusion.

This volume frankly “attempts, however inadequately, to fill the gap” (p. viii) left by R. H. Tawney's *Land and Labour in China* (New York 1932) which is now out of date. But the reason why Tawney's book has long been considered a classic is found in his firm grasp of the factual situation from various points of view, in his command of historical perspective, and in his un-

biased and well-balanced judgment. It will take quite a different volume from the present one under review to take its place.

CHOH-MING LI

*University of California, Berkeley*

*The Economics of Communist Eastern Europe.* By NICHOLAS SPULBER. (New York: Technology Press, Massachusetts Institute of Technology and John Wiley & Sons. 1957. Pp. xxviii, 525. \$12.50.)

Dr. Spulber's useful summary of postwar development in Bulgaria, Czechoslovakia, Hungary, Poland, Rumania and Yugoslavia<sup>1</sup> has three objectives: the encyclopedic task of describing what is known qualitatively about the area; the statistical task of assembling and interpreting fragmentary data prepared in ways often not acceptable to Western statisticians; and the analytical task of making the other two operations intelligible to the reader.

Spulber has admirably succeeded in the first task, presenting his data in such a way as to facilitate international comparisons, yet emphasizing the dangers of too glib generalizations, particularly with regard to timing of events. In particular, his treatment of Soviet assets in Eastern Europe (Ch. 6) is to be commended for bringing together much scattered data. As statistician, he has assembled the most important data for the years 1938, 1948, 1953, and 1955, together with plans for 1960; and has made numerous interperiod comparisons. The reader not wishing to wade through the entire East European Handbook Series (of the Mid-European Studies Center, to which Spulber is an important contributor) will certainly gain from the synthesis. The specialist might cavil at the summary treatment given to industrial financial controls (*Khozraschet*), materials balances, and controls over the flow of funds between city and countryside through wage and retail-trade operations (these are only institutionally described), but these may fall outside Spulber's analytical framework.

Analytically, Spulber considers that the focal issues in Eastern Europe (except in some cases for Yugoslavia) have been: (1) concentration of investment in processing facilities in industry, to the detriment of raw materials output, especially agricultural output; (2) suppression, on doctrinaire Marxist grounds, of private agriculture; (3) establishment of a foreign trade system which is an odd mixture of autarky, planning, bilateralism and Soviet colonialism. He views the general objectives of the governments as given or else changing slowly; and, although economic policies do change, they respond only slowly to the march of events. Thus after Stalin's death, there was the semblance of a new policy; but Spulber considers this a delayed response to internal pressures which had long been building up. Likewise he considers the return to more "Stalinist" policies in 1955-56 only the overt expression of what had always been the intent of the governments in these countries (and/or their Soviet masters).

<sup>1</sup> There are passing references to Albania, neglect of which is justified on the principle *de minimis*; the scant treatment of East Germany is more difficult to explain, since (despite vague hopes of German unification) this area is unfortunately part of the Soviet orbit. Yugoslavia has followed a course of development often differing sharply from that of the other countries. Thus, some confusion occasionally arises from his selection of countries.

This view regards the development plans (first about 1948; and again about 1955) as expressions of long-term intentions, and analyzes them in some detail (Ch. 8-9). If this conception of them is justifiable, then changes in policy in the interim can be regarded as incidental, and as adaptation to circumstance. There is, however, a basic difficulty in seeing such a plan as "a definite set of tasks which *must be* accomplished and which *are binding* as a law for society as a whole" (p. 282, italics added) if the statement that "the product mix obtained at the end of any plan period does not bear a close resemblance to the product mix scheduled" (p. 344) is correct. If the product mix differs, either the plan has changed, or it has been impossible to attain (*a fortiori*, not binding), or both. Actually, Spulber's evidence shows that plans were changed in 1950-51 (as a result, perhaps, of the outbreak of the Korean war); in 1953-54; and again in 1955-56. Were these changes in binding plans due solely to the pressure of events (pp. 354-56)?

Assuming, then, that there is something in a communist economy which may force the government to do something it does not wish to, the task is to locate what it is. Spulber shows us some obstacles: If collectivization proceeds too fast, private farms go out of business more rapidly than cooperatives can expand, so that acreage under cultivation drops (pp. 251, 265); given fixed prices, limited supply and increasing demand, rationing and dual pricing become necessary (p. 125 ff.), etc. However, there is no general statement as to the limitations which "scarce means" actually impose upon the desires of communist governments to expand heavy industry, collectivize agriculture, and have "good" relations with the USSR. The inquiring reader will ask how the limitations on the process of planning and controls actually have manifested themselves. It is perhaps too much to expect a complete answer yet, but to ask the question suggests a need for further exploration. It is to be hoped Spulber will later explain why "planning cannot accomplish . . . sustained growth at *expanding* rates" (p. 360, italics his); or, if "industrialization, *à la russe* could not be carried out 'administratively' by each and every country" (p. 362) just how Russian a country must be to play the Russian game (we will not call it roulette).

EDWARD AMES

*Purdue University*

*Western Enterprise in Indonesia and Malaya.* By G. C. ALLEN and A. G. DONNITHORNE. (New York: Macmillan. 1957. Pp. 321. \$5.75.)

The authors have added another volume to their studies on western enterprise in Asian countries. The present treatise follows rather closely the framework of the earlier study on China and Japan. However, while the first book emphasized the contrast in development between China and Japan, the countries now covered show many similarities. In Indonesia and Malaya modern development occurred under a colonial administration, both territories suffered from the effects of the business cycle and war devastation, both countries recently acquired independence. Their history is entirely different from that of either Japan or China.

In describing the pattern of development under the vagaries of world mar-

kets, war and politics, the authors have painstakingly tried to be descriptive rather than critical. Agricultural estates, mining, banking, shipping, public utilities, commerce, and manufacturing industries are dealt with in chronological order, comparing both territories sector by sector. In general, Indonesia is treated first; significantly, in mining the sequence is reversed.

The general conclusion following from this sectoral approach seems not unjustified—in Indonesia and especially on Java, the government fulfilled as (Western) entrepreneur a much more significant role than in Malaya. It may be questioned however, whether this was the result of a more *laissez-faire* attitude on the part of the Malayan administration. A distinction can be made between the policies followed in Indonesia—roughly speaking before 1870, from 1870 to 1913, and thereafter. In the first period, the government established a great variety of large enterprises, in the second period it left the initiative largely to private business, while in the third period an economic and social policy developed aiming to reconcile the interests of big business and the Asian masses. In Malaya, and likewise on Sumatra and Borneo, the pre-1870 period was skipped. As usual, latecomers are apt to be more modern, just as the policy on Java was modern when at the end of the eighteenth century any idea of slave-labor was discarded.

The comparisons would have profited and the underlying bias against "government-in-business" might have been further submerged, if the historic sequence and perspective had been emphasized more.

The motivation for more government intervention in economic life in Indonesia in the early part of the 19th century was the lack of a developed market for capital and products. After the separation of the budgets of Indonesia and the Netherlands, government ownership or participation in railroads, forestry, estate agriculture and mining was considered a method of local capital formation as well as a catalyzer for private investment. There was hardly a trace of a policy to keep private Western enterprise out; to the contrary, there were numerous measures designed to attract it. But at the time of the first world war social legislation developed rapidly, designed to protect the laborer and regulate relations between Western enterprise and Indonesian society. Similarly, an economic policy towards diversification and defense against the instability of export prices (it could not have been a countercyclical policy) was developed. The authors do not deny the existence of these general policies, but in view of their importance for Western enterprise they might have been highlighted.

The description of the effects of the second world war in both countries, of the difficulties experienced after the war and the prospects in independent Malaya and Indonesia are well written and the gloomy expectations for Western enterprise in Indonesia have an almost prophetic ring. As in the previous book, the authors display a lucidity of style and a balance of judgment which is impressive. The available material is so overwhelming that a careful choice of sources had to be made. On the whole the authors have succeeded extremely well in their task.

E. DE VRIES

*The Hague, Netherlands*



### Statistical Methods; Econometrics; Social Accounting

*The Soviet 1956 Statistical Handbook: A Commentary.* By NAUM JASNY.  
(East Lansing: Michigan State University Press. 1957. Pp. xii, 206.  
\$4.95.)

Compared to the trickle of data on the Soviet economy from 1937 to 1956 the published statistical information currently becoming available from Soviet sources has taken on flood proportions. A number of commentaries on these new handbooks have recently appeared.<sup>1</sup> In one of the more ambitious and rewarding of these commentaries Naum Jasny has provided a very useful critique of the new data as illustrated by the statistics available in the first, and one of the most important, of the statistical handbooks. But even in limiting himself to one handbook he has had to emphasize selected areas in his coverage. Agriculture, for example, is covered in great detail, whereas transportation is all but passed over.

A historical summary of Soviet statistical reporting forms a particularly interesting section of the book. In Jasny's view the current change in statistical reporting is more of a return to the methods of the 1930's than to the pre-industrialization period of the 1920's. It is also his opinion that the quantity of statistical data may have risen sharply but that the quality is still very low by either contemporary Western standards or Soviet standards during the 'twenties.

Likewise Jasny's page-by-page comments on the current statistical data which comprise the bulk of his book, make worth-while reading. Specifically, he points out omissions, shortcomings, and the appearance of new data, as well as suggesting interpretations of specific data. The attention he gives to this specific data is apparently governed by his own personal interest rather than by any objective standard of importance. He does not attempt to deal with the larger omissions in the over-all coverage of this handbook, nor does he attempt to interpret the new data in any broader context. We learn, for example, that horses are omitted from the category "productive livestock" (p. 118), but our attention is not drawn to the large gaps and omissions in Soviet reporting for such areas as foreign trade, defense, budgetary or fiscal matters.

Any economist interested in using Soviet data would do well to refer to the pertinent comments in Jasny's book before attempting a full analysis of the data involved. Although these comments may sometimes appear to include everything that came into the author's mind, the author's long experience with Soviet statistical material has nevertheless provided him with a wealth of information and many insights on a wide range of aspects of the Soviet economy.<sup>2</sup>

<sup>1</sup> See A. Nove, *Soviet Studies*, Oct. 1957, IX 127-28; A. Gaev, *Bull. Inst. Study of U.S.S.R.*, Aug. 1957, IV, 58-60; V. Marchenko, *Vestnik Institute*, No. 4, 1956, 82-88; *Economist*, July 20, 1957, CLXXXII, 229-31.

<sup>2</sup> Jasny's firsthand experience with Russian statistical data includes participation in the following activities: Economic Division of the All-Russian Union of Cities during the first world war; the Supreme Economic Council under the Kerensky regime, and work in Germany as a grain expert for the Soviet government during the 1920's.



As to the reliability of Soviet statistics, Jasny points out that "Soviet statistics are an amalgam of elements varying from trustworthy data (mostly pertaining to physical units or details) through ambiguities to obviously distorted estimates (mostly data from aggregates)" (p. 14). This reviewer would certainly agree that the data presented in physical units in these handbooks are much more reliable material and will probably be much more useful to the student of the Soviet economy than the other statistical measures which are given, particularly the aggregative measures in index number or value terms. Outstanding examples of unreliability are figures for the gross value of industrial output by year or by region and labor productivity for various years. Measures such as these are difficult to interpret, and at best can be used to represent orders of magnitude.

The difficulty of interpreting Soviet data is further aggravated by the lack of documentation or explanation of methodology. Any one who is accustomed to using the U. S. *Statistical Abstracts* would be surprised to learn that not one line in the Soviet handbook commented on by Jasny is devoted to source references or explanations of methodology. Thus, even the best data, the physical units of industrial output, must be interpreted with great care, especially if they are used to make international comparisons. One might be tempted to accept the Soviet comparisons of production levels of individual commodities such as steel, coal, electric power, and petroleum. But these measures do not reflect changes in quality over periods of time or internationally. Moreover, the Soviet policy of providing more data for the favored areas of heavy industry may lead one to assume greater over-all industrial growth than has actually occurred.

The statistical handbook reviewed by Jasny is but one of an increasing number of handbooks now available on the Soviet economy. A partial list of the formal handbooks is presented in Table I, but the new relaxation of restrictions on the issuance of statistical data is also reflected in many other Soviet publications. For example, no serious student of the Soviet economy should overlook the November issues of the Soviet industrial and economic journals, which provide data on the basis of a 40-year summary in commemoration of the October Revolution.

The above list could easily exceed 100 by the end of 1958 if all other organizations comparable to those listed in the table publish statistical handbooks. The number of handbooks is particularly inflated by the issuance of oblast data by the Central Statistical Agency's units in the oblast (province) and comparable administrative regions. Almost all the regional handbooks have a similar sector break-down and time coverage. Most of the handbooks have individual sections devoted to industry, agriculture, capital construction, transportation and communications, labor force, domestic force, education, and health. The years covered in most cases include 1940, 1945, 1950, and 1955.

A detailed survey of the contents of individual handbooks in this series is obviously impossible within the limited space of this review, but one comment seems quite appropriate. Although some new data appears in each handbook issued to date, the inclusion of much unnecessary and repetitious data considerably inflates their size. For example, nearly all presentation in

TABLE I.—SOVIET STATISTICAL HANDBOOKS  
(A Partial List)

Compiling Agency and Publisher	Title	Publication Date	Pages
<i>Sector and Industry Handbooks</i>			
*1. TsSU, Moscow#	Kul'turnoe stroitel'stvo S.S.S.R., st. sb. (Cultural Progress in the USSR)	1956	331
*2. TsSU, Moscow	Sovetskaia torgovlia; st. sb. (Soviet Trade)	1956	351
3. TsSU, Moscow	Zdravookhranenie v S.S.S.R. (Public Health in the USSR)	1957	178
*4. TsSU, Moscow	Promyshlennost' S.S.S.R.; st. sb. (Industry in the USSR)	1957	446
5. TsSU, Kiev	Zdravookhranenie v U(kr) S.S.S.R.; st. sb. (Ukrainian) (Public Health in the Ukraine)	1957	140
*6. TsSU, Moscow	Posevnye ploshchadi S.S.S.R.; st. sb. (Tilled Area in the USSR)	1957	2 v.
*7. TsSU, Moscow	Chislennost' skota v S.S.S.R.; st. sb. (Census of Livestock in the USSR)	1957	619
8. TsSU	Ugol'naiia promyshlennost' S.S.S.R., st. sb. (Coal Industry of the USSR)	1957	368
*9. TsSU, Moscow	Transport i Sviaz' (Transportation and Communications)	1957	259
<i>All-Union and Republic Handbooks</i>			
*1. TsSU, Moscow	Narodnoe khoziaistvo S.S.S.R.; statisticheskii sbornik (National Economy of the USSR; Statistical Handbook)	1956	262
*2. TsSU, Moscow	National Economy of the USSR; Statistical Handbook (English edition)	1957	230
*3. TsSU, Moscow	N.kh. R.S.F.S.R.; st. sb.	1957	370
*4. SU, Kiev	N.kh. Ukrainskoi S.S.R.; st. sb. (Ukrainian)	1957	534
*5. TsSU, Moscow	N.kh. S.S.S.R. v 1956 g.: st. ezhegodnik	1957	296
*6. SU, Kishinev	N.kh. Moldavskoi S.S.R.; st. sb.	1957	197
*7. SU, Ashkhabad	N.kh. Turkmenskoi S.S.R.; st. sb.	1957	171
*8. TsSU, Moscow	N.kh. Belorusskoi S.S.R.; st. sb.	1957	319
*9. TsSU, Moscow	Dostizheniia Sovetskoi vlasti za 40 let, v tsifrakh (Soviet Achievements During the Past Forty Years, in Figures)	1957	370
*10. SU, Frunze	N.kh. Kirgizskoi S.S.R.	1957	207
*11. SU, Riga	N.kh. Latvinskoi S.S.R.	1957	227
12. SU, Stalinabad	N.kh. Tadzhikskoi S.S.R.	1957	387
13. SU, Tallin	N.kh. Estonskoi S.S.R.	1957	307
14. SU, Tashkent	N.kh. Uzbekskoi S.S.R.	1957	197
<i>Handbooks for Cities, Oblasts, and Comparable Administrative Regions (by name of region only)</i>			
*1. Kostromskoi oblasti	*6. Stalingradskoi oblasti		
*2. Sverdlovskoi oblasti i goroda Sverdovska	*7. Cheliabinskoi oblasti		
*3. Ivanovskoi oblasti	*8. Tatarskoi ASSR		
*4. Kirovskoi oblasti	9. Iaroslavskaiia oblast'		
*5. Goroda Leningrada	*10. Buriat-Mongol'skoi ASSR		
	*11. Molotovskoi oblasti		

- |                            |   |
|----------------------------|---|
| 12. Zakarpatskoi oblasti   | *22. Irkutskoi oblasti                        |
| 13. Ul'ianovskoi oblasti   | 23. Kalininskoi oblasti                       |
| *14. Chkalovskoi oblasti   | 24. Komi ASSR                                 |
| 15. Karel'skoi ASSR        | 25. Kabardino-Balkarskoi ASSR                 |
| 16. Murmanskoi oblasti     | 26. Velikolukskoi oblasti                     |
| 17. Orlovskoi oblasti      | 27. Kuibyshevskoi oblasti i goroda Kuibysheva |
| *18. Leningradskoi oblasti | 28. Abkhazskoi ASSR                           |
| *19. Novosibirskoi oblasti | 29. Udmurtskoi ASSR                           |
| *20. Chuvashskoi ASSR      | 30. Adygeiskoi avtonomnoi oblasti             |
| 21. Arkhangel'skoi oblasti |   |

\* At the Library of Congress.

# TsSU refers to Tsentral'noe statisticheskoe upravlenie (Central Statistical Agency) attached to the Council of Ministers of the U.S.S.R.; SU to the appropriate regional office.

physical units is followed by needless percentage breakdowns. If in the regional handbooks attention were restricted to physical units of industrial production in 1955 and 1956, the latest years given, a handbook totaling some 300 pages might be reduced to a very small number of pages, in several cases to a single page.

In following the almost daily arrival of new handbooks at the Library of Congress a question recurring to this reviewer is: Why have the Soviets decided to release this new economic data? On this question Jasny concludes "Thus, the very manner of presenting the material stamps the *Handbook* as a propaganda publication" (p. 7). But propaganda as the sole or even primary explanation does not seem credible to this reviewer. Instead I would suggest a series of interrelated explanations drawn from Soviet and Western sources.

First, the new policy on availability of economic statistics may be a part of the "destalinization policy" or "thaw" in the Soviet body politic. This is a particularly difficult hypothesis to test or even to examine, as the relaxation in various aspects of Soviet domestic life following the death of Stalin has been very uneven since the initial period of rapid change. Moreover, the motivation and the actual degree of relaxation are often difficult to measure.

Second, the preparation of these handbooks may represent an effort on the part of the Soviet leaders to improve their own statistical reporting machinery. It may be true as is suggested by a number of Western writers, among them Jasny, that the State Planning Commission (*Gosplan*) has a complete set of statistics available for internal use. It may also be true, however, that the uniformity, comparability, and quality as measured by other statistical tests are poor for the data collected, especially from local reporting units.

The accumulation of reports from all the operating levels, which appears to be the procedure followed in issuing the new statistical handbooks, is a good initial approach to an improvement of the published statistics. A logical next step for Soviet statisticians would be to establish something on the order of the standard industrial classification system (S.I.C.) used in the United States. The S.I.C. was designed to bring about uniformity and comparability of reporting of all economic activity in the United States, the lack of which in Soviet statistical reporting is evidenced by these statistical handbooks.

Third, Khrushchev's decentralization program apparently calls for a larger share in the planning and operation of the economy at lower administrative levels. Detailed reporting of statistical data by oblast and city may be considered a necessary first step for greater participation of these administrative units in the operation of certain economic sectors. The fuller reporting in these regional handbooks of industries such as timber, fuel, and agricultural products suggests that the coverage coincides with the economic activities in which the role of regional planning is intended to increase. It is of interest that nearly all the oblast and ASSR handbooks currently available are from the RSFSR, the largest of the Soviet republics. Most of these administrative units coincide geographically with the new regions of the economic councils (*Sovnarkhozy*) under Khrushchev's decentralization program. The economic regions also generally coincide with the oblasts of the Ukrainian republic, although few handbooks are as yet available from Ukrainian oblasts.

At the same time, some reports indicate that the new decentralization would paradoxically result in more central control. However, as some of these reports were made by officials such as Zverev, Minister of Finance, they may indicate a centralization of *financial* control as a concomitant of relaxed control on *physical* production.

Fourth, competition between regions is a significant factor. This theme is receiving considerable attention in the Soviet press and may be part of an effort to exhort the Soviet people and responsible local leaders to greater efforts. As pointed out by A. Nove, data unfavorable to the Soviet Union in terms of international comparisons such as for relative consumer goods production are presented in each of these handbooks.<sup>3</sup> The appearance of unfavorable data might be explained as laying the basis for a later claim of overtaking the U. S. for various production levels.<sup>4</sup> Or it may be a method employed to spur on the local party secretaries, plant managers, etc.

Fifth, the increased availability of statistics is probably intended to increase the quality of Soviet research and theoretical debate. The very interesting discussions on long-term growth and the theory of value currently appearing in the Soviet press are surely encouraged and aided by the availability of more published data.

Finally, pride in national accomplishments may be cited as a rationale for the new availability of economic data. Some aspects of Soviet economic growth have, in fact, been remarkable, and desire to present these accomplishments in statistical form both for the world and for their own people was undoubtedly a factor in the decision to make available more statistical data on Soviet economic development. Closely related to this theme is that of propaganda, as it can hardly be contended that the Soviet leaders are attempting to place an accurate picture of the Soviet economy on the record. Still, if propaganda were the primary consideration, it would have been more logical

<sup>3</sup> A. Nove in an unpublished commentary on *Socialist and Capitalist Countries in Figures*.

<sup>4</sup> Suggested by D. Gale Johnson in comments on Soviet agricultural production at the meeting of the American Economic Association, December 29, 1957.

for the Soviets to prefer a smaller number of titles with larger circulation and translated into various foreign languages.

Economists interested in the Soviet Union may be thankful for whatever additional assistance they get in their research. However, the increased availability of data does not appear to be sufficient to warrant a basic shift in the use of the Soviet statistics from the painstaking, detailed type of examination and interpretation necessary to date. Some greater emphasis on general policy questions based on the accumulated work of Western scholars and the additional data now available will certainly be possible, but only after careful examination and interpretation of the data.

JOHN P. HARDT

*Corporation for Economic &  
Industrial Research  
Arlington, Virginia*

*Quantity and Price Indexes in National Accounts.* By RICHARD STONE. (Paris: Organisation for European Economic Cooperation. 1956. Pp. 120. \$1.50.)

In recent years more comprehensive and consistent approaches to index number construction and interpretation have been evolving in this country and abroad in good part as a result of the development of national accounting statistics. This development has been fostered by the United Nations and the Organisation for European Economic Cooperation by their recommendations on how statistics on production, prices, and national income and expenditures should be developed and presented. Professor Richard Stone has been a leading participant in work by both organizations, and his present report on quantity and price indexes done for the OEEC is a significant contribution to the subject of measurement of "real" output of the national economy.

While Stone's style is likely to seem forbidding to all but a relatively small group in the field of index number and national accounting methodology, his report has broad implications for important statistical programs in this and other countries. Stone deals mainly with two questions. One involves construction of a consistent set of price and quantity index numbers for the total economy. The other relates to an ever-present challenge in index-number construction—measurement of changes in quality.

In order to achieve consistency in a system of price and quantity indexes for total national economic activity, Stone contends that the accounting of this total in "real" terms should be confined to "commodity transactions." Commodities are defined to include goods and services, other than factor services. Thus Stone suggests that for a consistent presentation of national output statistics in terms of constant prices two approaches are appropriate. One is the value-added framework, showing the goods and services supplied by each industry. The other is the expenditure framework showing the final value of goods and services purchased by broad classes of consumers including households, business, and government. With a consistent basis of valuation, the value-added and expenditure frameworks yield equal aggregates, apart from errors in data.

The different kinds of detail provided by each framework serve different analytical purposes. A presentation of the industrial origin of goods and services helps in analysis of supply developments and of the use of resources including use of fuel, manpower, and electric power. The detail in the expenditure framework lends itself more to study of demand, and in approximate fashion to study of welfare. Work with both frameworks helps to verify and make more coherent the study of supply and demand developments in the economy. Also, both frameworks can be systematically integrated in input-output tables showing the sources and uses of national product.

The changing values in current prices shown for the components of each framework, when divided by the corresponding values in constant prices, yield implicit price indexes or "deflators." These three sets of data—current price series, constant price series and "deflators"—are, in Stone's view, the basis for systematic analysis of price and quantity changes throughout the economy. On this latter point, however, differences of view are likely to develop because of possible confusion between regularly compiled price indexes and these implicit measures.

In this country, work on regular publication of national accounting statistics in real terms is in various stages of development. On the final expenditure side, the gross national product statistics in constant dollars are published annually with some prospect of quarterly publication. On the side of value-added industry measures—several major sectors of this framework are regularly published monthly including an index of industrial production (manufacturing and mining), an index for electric and gas utilities, and an index of construction activity. In addition, various indexes of agricultural output are available annually. Unofficial annual totals for the private economy, including measures for trade, transportation, and services have been prepared. Also, for the manufacturing sector, work has been done for the years 1947 and 1949 through 1953 on "net output" indexes involving a deflation of industry inputs (except factor inputs) and outputs with net output derived by subtraction.

In recognition of the importance of real output measures, the *Economic Report of the President* for 1958 headed its list of recommendations for a program of improved federal statistics with proposals for quarterly estimates of gross national product and its major components in constant prices and for measurement in constant prices of net output for several major industry groups. Stone's report does not seek to answer all the detailed questions that would be involved in implementing these recommendations. He does distinguish by implication between short-term measures and longer-term measures. More systematic treatment on this point would have been desirable. Thus Stone's discussion of the detail in the data required for full analysis of commodity flows, for adjustment to reflect quality changes, and for input-output analysis, would have benefited by a distinction between what might be done on a current basis (monthly, quarterly, or even annually) as against what might be accomplished more completely at "benchmark" dates.

This distinction is particularly relevant in connection with Stone's suggestions for adjustment for quality changes. As is well known, individual products often treated as identical tend to improve in quality because of changes



in functions, design or other specifications. Sometimes, such changes can be measured with reasonable accuracy if sufficient detail by type of product is available. However, even considerable detail does not help when the bulk of the individual items made are in fact different in different periods. In such a case, changes in value may result from changes in quality as well as in price or in the simple count of the quantities produced.

Stone suggests that quality differences can be measured if information can be obtained on a set of specifications which can "explain" price (*i.e.*, quality) differences among different grades of a product in the base period. For example, if poundage and horsepower could explain nearly all of the price differences among autos of different make and design, then an appropriate weighting of these two specifications might account for changes in quality. Such a measure might be superior to a simple unit count of autos. It does involve the assumption, however, that these specifications would be the chief source of *change* in quality of individual makes and models. This is a difficult question, and like Stone's discussion of seasonal adjustment problems, might have been more suitably put in a technical supplement.

There is full recognition in Stone's report of the need for a flexible and eclectic approach to the problem of measurement. The need to exercise judgment and ingenuity in the use of imperfect data is clear. Those familiar with measurement problems realize how much heroic effort goes into the job of estimating the facts. In the United States it should not be necessary for compilers of official indexes to make ingenious estimates of output for such key areas as producers' and military equipment. While conceptual or index-number problems of valuation will always be very perplexing in this area, the problem is in good part perhaps a practical one of obtaining the basic information. The answer seems, to this reviewer, to lie in giving greater prestige and importance to the need for a more adequate factual record of economic growth. The reports by international organizations, such as the report by Stone, should be expected to further this objective.

MILTON MOSS\*

*Board of Governors of the Federal Reserve System  
Washington, D. C.*

\*The views expressed in this review are those of the author and do not necessarily reflect those of the Board.

### **Economic Systems; Planning and Reform; Cooperation**

*Factory and Manager in the USSR.* By J. S. BERLINER. (Cambridge: Harvard University Press. 1957. Pp. xv, 386. \$7.50.)

The principal character in this book, the factory manager, is central to the question of the operating effectiveness of the Soviet economy, because upon his shoulders rests the primary responsibility for translating the formalities of the state economic plans into concrete decisions and specific activities. But the very nature of his position presents a complex and obscure picture to the analyst. The fact that Professor Berliner has succeeded in bringing many individual aspects as well as the over-all view of the Soviet manager



into sharper focus therefore makes this an important book and one that will interest a wide range of readers.

The book opens with a lucid summary of Soviet economic institutions in so far as they form the operating milieu for the factory manager, following which the "goals of management" are discussed. In developing this subject at some length, the view is put forth that, of all considerations which might be termed "goals," the desire for money premiums stands clearly to the fore. I shall comment on this conclusion below.

The largest portion of the book is devoted to an elaboration of the operating techniques employed by the Soviet manager in attaining his goals. In addition to techniques *pro forma* acceptable to the authorities, "three leading principles of action" are cited which, although in varying degrees unacceptable officially, appear to be in fairly wide application: (1) the cultivation of a "safety factor" in production and procurement; (2) the alteration of the "assortment of production," either in quantity or quality, or through the falsification of results; and (3) the use of activities combining "illegality with personal influence."

The final section of the book (aside from a summary chapter and one on post-Stalin reforms) is devoted to the matter of controls over management. Berliner weaves into a discussion of the formal aspects of control, involving the ministerial apparatus, the Communist party and the trade unions, the essential features of what he calls "mutual involvement" between management and the control representatives, a condition which goes some way to explain the continued existence of the illegal or semilegal activities outlined in the preceding chapters.

Many of the aspects of Soviet managerial life discussed in this study have appeared in other studies, notably that of David Granick.<sup>1</sup> What is unique in this instance is partly the inclusion of the postwar period, but mostly the novelty of the sources, which comprise in addition to Soviet printed materials (of a type also used in other studies), the testimony of forty-one former Soviet citizens with experience in various managerial positions prior to their defection following the second world war. These first-hand sources give the author insights not otherwise obtainable; but at the same time, broad areas of similarity remain in comparison with other studies. The defector materials do not so much reveal aspects of managerial behavior heretofore completely unnoticed, as they permit a reordering of emphasis and the injection of considerably more depth and "personality" into the presentation.

The use of a relatively small number of defectors raises methodological questions which the author discusses at some length, concluding that the testimony is "reliable," given the structure of the interviews and the analysis to which the results were subjected. Along the same lines, however, it is interesting to note that, although this is a study of the "factory" and the "manager" in the U.S.S.R., not one of the forty-one informants had ever been a manager of a factory. The closest resemblance involves one director of a construction trust and one chief of the regional office of the Commissariat of Procurement, while the higher echelons of factory management are represented by six chief

<sup>1</sup> D. Granick, *Management of the Industrial Firm in the U.S.S.R.*, New York 1954.

engineers and a number of department heads. Having in mind that the author's basic objective is to portray "what it is like to be a manager of a Soviet industrial enterprise" (p. 1), and recalling that the manager himself, as distinct from all other individuals in the management complex, is responsible for decision-making, I am not sure that the absence of bona fide factory managers among the defectors does not qualify the usefulness of the materials to a certain extent. Apparently Berliner assumes otherwise, however, when he observes that he is (perforce) interested in these individuals "not as 'average respondents' representative of a parent statistical population, but as 'expert informants' capable of . . . reporting objectively on their own and others' experiences" (p. 8).

The interview materials are most effective in the examination of operating techniques, but they are also used to support a re-evaluation of the "goals of management." The conclusion is that money premiums are the "dominant goal [that] more than anything else stands at the forefront in the making of decisions and explains why one alternative is chosen in preference to another" (p. 43). I am not completely certain what this is meant to convey as an indicator of basic managerial motivation. It is a fact that premiums have been intended primarily to reinforce "plan fulfillment and overfulfillment," and that the latter phrase encompasses what is generally considered (by Berliner and others) to be what the authorities reckon as the most desirable managerial objective. In other words, premiums have been designed to achieve a relatively specific purpose, as distinct from representing in and of themselves the net effect of all managerial activities, in the sense of "profits" under capitalism.

Subject to this limitation, the seeking after premiums is officially respectable behavior, however much it reflects basic motivation on the part of the manager. But Berliner seems to be suggesting something even stronger; he seems to suggest that there is a tendency for the manager to regard premiums purely as ends in themselves. Unfortunately, this is difficult to prove, because premiums and "plan fulfillment" pull in the same direction most of the time. In any event, I question the idea, at least as far as the "successful" manager is concerned. I would not expect him to become absorbed in the quest for premiums to the exclusion of their "real" counterparts, because if he did, he would make himself psychologically vulnerable in a way inconsistent with success as a manager.

WARREN W. EASON

*Princeton University*

### Business Fluctuations

*Monetary and Fiscal Policy under Uncertainty.* By KARL-OLOF FAXÉN.  
Stockholm Economic Studies, n.s. 1. (Stockholm: Almqvist and Wicksell.  
1957. Pp. 212. SKr. 20.)

The fiscal policy examined by Faxén is represented by the following government parameters of action: public expenditures (for purchases from business) and (proportional) income taxes on wage earners and business firms; as to

*monetary* policy, Faxén considers actions influencing the interest rate and those directly influencing firms' borrowing. The various models for the economy which he discusses have in common: (a) a simple consumption function (consumption as function of "yesterday's" disposable income), (b) an oligopolistic price-wage relation according to which price is a multiple of the wage rate, (c) a particular production function relating output to labor input in the same period and shifting according to the changes in the stock of capital and the—imperfect and slow—interindustrial and technological adjustments. The various models also have in common the following unknowns: (1) the quantity of production, (2) the price level of products, (3) the volume of employment, (4) the wage level (all four in index form), (5) the gross value of business investment, and (6-8) changes in the national debt and in the financial assets of the business sector as well as of the wage earners.

Any system of difference equations in which both price and quantity of output are to be determined, is essentially nonlinear, and can be solved only in exceptional cases. Faxén succeeds, however, by extreme simplifications (zero interest and dividend payments, constant investment, and taking the business sector's financial assets as a fixed proportion of sales) in rewriting the equations for employment and prices in such a way that besides the government's parameters of action they contain only past values of the variables. The price and wage level proves not to be influenced by business taxation; hence it can be controlled by government action independently of the volume of employment. If, on the other hand, the situation in the labor market is assumed to influence wages (and indirectly prices), the price-wage level and the volume of employment cannot be fixed separately; in other words, full employment implies higher prices than underemployment, and extraneous tendencies to rising wages and prices cannot be counteracted by government policy without employment also being affected.

To go beyond this result, Faxén works out some numerical examples for an underemployment economy, under the following assumptions: firms are supposed to face the alternative, in the first half of the period, of either maintaining the current rate of investment of 25 units, or increasing it to 27 units; and in the second half of the period firms face the alternative of either investing 27 units or increasing it further to 29 units. The investment decisions for the second subperiod are influenced by government policy, for which, in turn, two alternatives are considered: whether in the second subperiod the government imposes heavy quantitative credit restrictions or higher taxes on households. Examining the effect of the policy chosen on income and what he calls the "liquidity" of business, Faxén establishes a certain order of preference for the seven possible alternatives; naturally, processes leading to overemployment (hence inflation) or, at the opposite end, processes not eliminating underemployment get a low mark; furthermore the government may be presumed to strive for the largest possible volume of investment following a steady course of development, as Faxén somewhat cryptically expressed it. Thus the government would prefer the firms to produce 27 units in both periods, but this combination does not prove the most profitable for the producers regardless of whether quantitative credit restrictions or higher taxes

on households are imposed. However, by pursuing the latter policy the government can realize the second-best combination, 27 units in the first period and 29 in the second one; investment is higher and consumption is lower than under the first combination.

While the result is acceptable from a common-sense angle, it cannot be considered proved—an inevitable outcome of an analysis based on a numerical illustration. Is it certain that no more can be done? To attain his results, Faxén had to resort to further drastic simplifications: the increase in the stock of capital is not supposed to affect prices and output in succeeding periods, nor will the investment process slacken in periods of declining utilization and consumption. The dynamic character of the process is ensured by retaining the consumption time-lag—no other dynamic features occur; and for the wage earners this time-lag has been shown by Tinbergen to be negligible. Had Faxén discarded also this last dynamic element and, where necessary, chosen for his production function a linear approximation, more general results might have been obtained both for the general model and for the modified model.

The book contains interesting supplementary studies on planning by business enterprise and on wage policy. For lack of space we limit ourselves to a brief comment on the former. Here particular attention is paid to the "self-financing" policy of firms. If the oligopolistic firm's expansion depends on self-financing and a fixed relation exists between the stock of capital and the rate of production, there is an optimum price policy for the firms, depending, *inter alia*, also on the reactions of the competing oligopolists. The graphical analysis does not seem in this case to entail a serious loss of generality; it is extended by Faxén to include the effect of government tax policy, interest rate policy and partial self-financing. He also believes that the results obtained by micro-analysis of the firm can be made applicable in macro-analysis to aggregated business sectors, but does not explain the meaning of aggregation of demand functions for the various products of individual firms, though each of the functions could be set up only under the assumption of given price and wage policy of the competing firms.

While the principal analysis of the study could have been fruitfully expanded in various directions,<sup>1</sup> the lengthy methodological chapters comprising almost half of the book could have been substantially condensed or omitted altogether. We are informed by now about the disequilibrium approach of the Stockholm School; the graphical presentation of alternative policies in "partition trees" is understandable even for a reader who skips Chapter 3, and moves directly from the Introduction to Chapters 4 and 5.

HANS NEISSER

### *New School for Social Research*

<sup>1</sup> The identities in the general model also require careful checking. In identity (1) the sales value of products is put equal to the sum of the wage bill, gross investment, business taxes and change in "liquidity." Suppose a firm does not sell anything during a given period but produces for inventory: would it have to compensate by net changes in financial assets (in "liquidity") both the wages paid and inventory investment during the period? In other words, should not inventory investment have been excluded from the gross value of business investment, contrary to his statement (p. 113)?

### Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

*The Federal Funds Market: Its Origin and Development.* By PARKER B. WILLIS. (Boston: Federal Reserve Bank of Boston. 1957. Pp. 39.)

It would be hard to find many definite references in the general literature to the nature and significance of the federal funds market. Partly as a consequence, the growing importance of the federal funds rate as a key policy instrument has been overlooked in many quarters. What Parker Willis has done for the layman in this little book is to spell out the role of "a sensitive indicator of shifting pressures in the banking system" of the United States.

One cannot come away from a reading of this excellent little book without a greater appreciation for the increased complexity of open-market policy in recent years and the vastly enlarged possibilities for flexibility in the administration of that policy.

But what Willis does not say is in its way just as important and it remains to be said. Here is the one market where is determined currently the one rate that is now being thought of as the "key" rate. Since by nature, it is a "residual" market, the funds market tends to reflect the changing impact of business conditions and policy pronouncements more readily than any other. But what are the implications of this "new" market for Federal Reserve policy? Are current policy instruments adequate for their assigned task?

Moreover, little is said about the possible evolution of the funds market; for it seems certain that today's organization is in the process of dramatic change. On the one hand, banks, particularly those in the larger centers, may turn to funds transactions more and more as they find this market more convenient and more pervasive. On the other hand, this market which up to now has been dominated by non-bank firms may become a bank-controlled entity.

Willis provides a basis for understanding the nature of the funds markets. He does a less effective job in describing the possible role of that market. And he leaves his reader rather in the dark when an explanation seems necessary to account for concern of the Federal Reserve Board over the current structure of policy instruments. But his *Federal Funds Market* is an essential part of the general literature on money markets.

CHARLES J. STOKES

*Atlantic Union College*

### International Economics

*La Zone Sterling.* By JEAN DE SAILLY. (Paris: Armand Colin. 1957. Pp. xviii, 131.)

The persistence of the sterling area as a regional monetary arrangement since 1939 has given rise to a growing body of economic literature. A recent

addition is the booklet of some 130 pages presenting an "over-all" view of the sterling system, by Jean de Sailly.

The author's approach reflects his basic concern with an evaluation of the relative effectiveness of the British and French regional monetary systems in coping with problems of achieving balance-of-payments equilibrium. De Sailly regards the entire twentieth-century British experience as a basically successful application of extraordinary flexibility and judgment by successive governments in meeting recurring crises in the international monetary realm. The persistence of the comprehensive arrangements of the sterling area without detailed written agreements and elaborate formal steering machinery is considered a remarkable achievement.

The system tends to strengthen the economic cohesion of the area. Through reinforcing the imperial preference system, exchange restrictions buttress the traditional complementary relation existing between industrially and financially advanced Britain and the raw-material-producing Overseas Sterling Area. Moreover, the accumulation of blocked balances by the colonies allowed the metropolis to resume investment in the Independent Sterling Area. Without this flow, the export of capital goods would have been impeded and the pressure to accelerate imports from the United States aggravated.

Yet de Sailly is aware of the formidable character of the stresses and strains afflicting the system. Without dollar aid, the British would have been unable to generate over-all balance-of-payments surpluses necessary to sustain the export of capital to the independent members of the area. Since 1955, domestic inflationary pressures have restricted the magnitude of this flow. India not only has been a recipient of British investment but has drastically reduced the size of its sterling assets accumulated during the war. Yet her development programs have required increased financing from the United States and the World Bank. Finally, as the colonies achieve a greater measure of independence, it seems unlikely that they will continue to augment their sterling balances by exporting to the dollar area (especially when the dollar values of the balances could be adversely affected by devaluation) and to purchase British rather than American equipment.

The establishment of current-account convertibility at fixed rates of exchange with the elimination of the distinction between American-account and transferable-account sterling presents serious hazards. It would foreshadow increased competition with the United States not only within but outside the sterling area. The mitigation of the risks associated with this move would require the unlikely achievement of the following combination of conditions: a "sufficient" level of dollar and gold reserves, the adoption of an enlightened commercial policy by the United States, and equilibrium in the balance of payments for the sterling area. In the long run, a significant expansion in the British capacity to export will be required when the maintenance of full employment and rising living standards are "perfectly anchored in the British mentality" (p. 123). Despite the profound and continuing nature of these problems de Sailly appears confident that the flexible nature of the sterling area will produce more effective solutions than the French system.



Yet this conclusion reflects both the strength and weakness of the work. Nowhere do we find an examination of the criticisms of the functioning of the sterling system raised by Bell and Zupnick. In his descriptive treatment of the major postwar problems, de Sailly avoids considering whether more effective coordination would have lessened the severity of the recurring crises. Extended discussion, moreover, is lacking of the fundamental problems confronting Great Britain in achieving a more desirable allocation of output between exports to the dollar area, the sterling area and the rest of the world, and domestic investment. It is easy to criticize a short work for acts of omission, which partially reflect differences in emphasis and approach. Although no original information is provided, this booklet should be interesting to those seeking a synthesis by a French student of the international economics of the sterling area.

MATTHEW SIMON

*Pace College*

*L'Echange international*. By MICHEL MORET. (Paris: M. Rivière & Cie. 1957. Pp. 371. 1,350 fr.)

The main objective of this study is to review the present status of international trade theory. The first part deals with the classical theory of international trade. Moret then explains that the modern theory has developed in two principal directions. On the one hand, there have been the attempts to improve the classical theory and to adapt it to realities and new circumstances. Among these improvements the author mentions various refinements concerning the terms of trade and their calculation, and attempts to evaluate properly the probable effects of devaluation. Ample use is made of econometrics, and the problems of elasticity of demand and supply and of multiplier are discussed.

Other economists have tried to break completely with the past and begin with a new foundation. The income theory which is linked with national accounts is prominent in the new international trade theory and the needed adjustment in the balance of payments is derived not from interplay of prices but from changes in national expenditure. In this connection the author touches on international economic relations within the framework of economic growth.

Moret sees the main weakness of the present state of the subject in the fact that the two principal theories ignore each other. Thus a new orientation in research is proposed which should attempt a synthesis of the price and income approaches, and should study the effects of economic domination and the problem of centrally controlled economies in international relations.

In his conclusions Moret quotes Jacob Viner who, while insisting on the necessity of improving the present theory, expresses doubts that a general theory can be worked out. He says that the classical theory cannot be directly applied and perhaps no adequate general theory can be developed for the world of today. Moret, however, thinks that the economic theorist should become the guide of external economic policy. To that end he should determine the limits of a theory on the basis of which policy-guidance could be given.



Appended to the book are some very interesting observations by J. Weiller dealing with the transition from theories to policies of international trade. According to Weiller, the theory of economic policies must be adjusted to circumstances if adequate results are to be achieved. He illustrates his point in discussing problems of underdeveloped countries, and the questions related to economic growth and touches on the complex problem of fundamental disequilibrium. Weiller is in favor of working out a theory of international economic policies. This reviewer agrees with his comment that contemporary authors complicate schemes which had already been too abstract and transform them into models with numerous variables. Very often such an approach does not prove very helpful in dealing with concrete situations.

The book also has a very stimulating foreword by C. P. Kindleberger who deals mainly with the fields in which future research should concentrate. Of seven items proposed by him the following seem very important: economic integration in world economy; the possibility of applying the theory of foreign trade to underdeveloped countries; effects of foreign trade on income distribution within a country and between countries. Kindleberger rightly stresses that international trade theory needs to be in closer touch with the facts of real life and he concludes that we are only beginning to understand the mechanism of international economy in theory and in practice.

The book by Moret and the comments by Weiller and Kindleberger agree on one item—the time has passed when the theory of international trade focused on so-called equilibrium and outlined which adjustment was needed to restore a disrupted equilibrium. The term “adjustment” was a universal formula which would cover everything. This theory operated in a vacuum, often not examining the political and social complications of any major adjustment and not taking into account important structural changes which many economies had experienced. After all, it would have been possible to produce arguments that an adjustment as postulated by the classical theory could have solved even the postwar dollar problem.

Whether a new general theory of international economics will emerge is perhaps not too important because any such theory would have to be based on many assumptions which would need to be qualified or adjusted so as to fit any real situation. It is much more important to study the multiplicity of problems of the present international economy, to clarify the various concepts and to try to find what might be considered the most acceptable solutions. Moret's study is one of those which can stimulate future research showing the shortcomings of the past and present theories and the ever-growing gap which should be closed.

ANTONIN BASCH

*New Delhi, India*

*International and Interregional Economics.* By SEYMOUR E. HARRIS. (New York: McGraw-Hill. 1957. Pp. xiv, 564. \$7.00.)

This book might be likened to a Harris tweed. Its quality is high but its finish is rough. It deals with a series of topics such as might be selected for a

graduate course in international economics. One of the topics, the dollar shortage, is treated as a unit and constitutes more than half the book. The other topics appear almost as a series of readings, each one interesting but not thoroughly integrated with what precedes or follows. This may be a "hand-book" but it is not a treatise.

Seven chapters in Part I deal primarily with the classical position on the distribution of output among nations. The quotations from classical authors are pertinent and the author's comments penetrating, but the reader feels adrift until he reaches the summary in Chapter 7. The introduction to Part I does not provide a theme sufficient to hold together the range of material presented. Coverage, of course, is incomplete. For example, in the discussion of the gains from trade, the two-country, two-commodity case is presented and the multicountry case is introduced, but Graham's criticism of Mill's "limbo" ratio is not mentioned. Throughout this section, the imminent presence of Haberler is felt by the reader as it must have been by the author's students.

The six chapters of Part II trace the development of monetary policy from the days of the classical economists to the present. The material for the earlier period is in uneven detail, and when taken together with the author's asides, the reader is left with a cloudy perspective. The two chapters on recent objectives and policy are well done—fresh and astringent.

Part III, one chapter on regional economics, makes perceptive observations on the regional aspects of imports and exports of the United States. As an example of the small irrelevancies that abound in the book, however, this chapter ends with a plea for a revised federal spending policy for New England.

Parts IV and V contain fifteen chapters on the adjustment process, with special reference to the "range of problems subsumed under the generic term 'dollar shortage'." This section might have been a separate book on the subject, and it will be unfortunate if the title of this volume diverts the attention of the student or the general reader from its excellent discussion of the world dollar problem. Chapters 22 and 23 put the United States tariff in proper perspective as an "enemy of the people" but not in number one position. The statement that the philosophy of the trade agreements program should be discarded, with policy emphasis on "more imports, not more imports *and* more exports," seems to be at variance with the emphasis in Part I on the importance of economic allocation of resources, and to place the solution to the dollar shortage in first position as a present-day problem. A summary of the controversy over an increase in the price of gold concludes with arguments opposed to this move that are relevant to an inflationary 1957. Will they be relevant in 1959? An interesting chapter on convertibility suggests that a gold and dollar reserve for Great Britain of \$4 to \$5 billion, and a rate of exchange "floating" within a 10 to 15 per cent range with stabilization-fund support, might make a restoration of convertibility possible, especially if most of the inconvertible currencies took the step together.

This is not a book to be picked up by a beginner. Parts I-III, particularly,

will serve best as a reference, supplementing organized lectures or readings. Parts IV and V constitute a review of literature and interpretation of facts that require prior acquaintance with the problem of dollar shortage. Properly used, the book will undoubtedly contribute much to other seminars, as it must have to the one out of which it grew.

SAMUEL E. BRADEN

*Indiana University*

### **Business Finance; Investment and Security Markets; Insurance**

*Corporate Finance.* By ELVIN F. DONALDSON. (New York: Ronald Press. 1957. Pp. x, 876. \$7.50.)

This well-organized and readable book gives a wide choice of assignments for a one-semester course. The text is well suited for what might be called the "orthodox course in corporation finance"; it is also suited for a curriculum that does not offer a course in investments, and must combine this material in the one finance course. There are chapters on investment banking, regulation of security issues, buying and selling listed securities (with an account of exchanges, the over-the-counter market, short selling, margin accounts); and a chapter on investment companies, where a careful distinction is made between a closed-end and an open-end company.

A full discussion is given on the market price of closed-end company shares and charges for buying shares of open-end companies. The monthly investment plan of the New York Stock Exchange and dollar averaging are explained. A clear and well-developed chapter is given to privileged subscriptions.

The book is divided into seven parts: Forms of Business Organizations; Corporate Securities; Promotion and Financing Through Securities; Working Capital; Administration of Income; Expansion and Combination; and Readjustment, Reorganization, Receivership and Dissolution.

The author's knowledge in the field of business organization, which made his earlier text *Business Organization and Procedure* a real contribution to the field, helps relate the material, brings in specific examples, and points up the principles underlying the modern business corporation's financial policy.

Partnerships are covered more fully than in most texts, employee ownership of securities is discussed, depreciation according to the Revenue Code of 1954 is covered, under- and overcapitalization are well explained, and inventory valuation is presented. The choice of policy in relation to taxation is emphasized.

Questions and problems are at the end of each chapter and an instructor's manual prepared by the author is available. There is an excellent bibliography of selected readings. It is a long book, but this reviewer is so impressed with its "teachableness" that he is adopting it as his text next semester.

GEORGE W. SANFORD

*Case Institute of Technology*

**Industrial Organization; Government and Business;  
Industry Studies**

*Antitrust in Perspective: The Complementary Roles of Rule and Discretion.*

By MILTON HANDLER. (New York: Columbia University Press. 1957. Pp. iii, 151. \$3.00.)

This monograph had its origin in three guest lectures delivered in 1956 at the University of Buffalo School of Law. The first chapter recounts the development and application of the "rule of reason" in the interpretation of the Sherman Antitrust Act. Whereas that act forbids "every contract . . . in restraint of trade" and "every . . . attempt to monopolize," it has long since been construed to forbid only unreasonable restraints, *i.e.*, those with significant deleterious consequences. The second chapter deals with the interpretation of the condition attached to the Clayton Act proscription of tying clauses and exclusive dealerships "where the effect . . . may be to substantially lessen competition or tend to create a monopoly in any line of commerce." The final chapter deals with the interpretation of similar conditions to the proscription of corporate mergers under the original Clayton Act and the 1950 Celler-Kefauver Act.<sup>1</sup> Over half the volume is given over to footnotes which, in addition to citing court opinions (and dissents), present many illuminating insights into Handler's views.

Like other attorneys representing antitrust defendants, Handler is an eloquent opponent of any extension of the doctrine that certain acts always, or under specified conditions, are in themselves (*per se*) unlawful.<sup>2</sup> One of his principal targets is the position taken by the Supreme Court in the last ten years that the Clayton Act is violated by any tie-in that foreclosed a substantial amount of business (the "quantitative substantiality" rule). This, he argues, is inconsistent with the Court's application of the antitrust statutes in somewhat similar situations; furthermore, he asserts that on occasion the rule prevents a practice that would enhance competition.

Handler has similar misgivings with respect to the test of the probable competitive effects of a merger which the Court employed in determining that DuPont must divest its stock holdings in General Motors. In this case the (original) Clayton Act was held to have been violated because the stock acquisition established a likelihood that competition may be foreclosed in a substantial share of a substantial market.<sup>3</sup> Handler asserts that this test

<sup>1</sup> For corporations "subject to the jurisdiction of the Federal Trade Commission" the Celler-Kefauver Act replaces the merger section of the Clayton Act. The new law applies to acquisitions of assets as well as stock and phrases somewhat differently the test of probable competitive effects. The original Clayton Act provisions remain in effect with respect to corporations such as banks, meat packers and interstate motor carriers.

<sup>2</sup> He concedes that "*per se* doctrines are appropriate where trade practices, like price fixing, are inherently antithetical to the precepts of a competitive order" (p. 69).

<sup>3</sup> This decision was handed down by a four-member majority, with two members dissenting. Justices Clark and Harlan took no part, because of previous connection with the case. Justice Whittaker was appointed to the court after the case was argued. In the briefs little attention was given the Clayton Act count. Handler suggests that in view of the various special circumstances subsequent cases may have a different result.

jeopardizes "thousands of acquisitions that have taken place since 1950," and implies that many such mergers either are without competitive effects or "invigorate the competitive process." Consequently, he calls for "a comprehensive analysis . . . based on an awareness of all relevant market data."

Three observations are in order. A rule of reason has merits, but only in a most general sense can it be said to provide "one essential standard" for the application of the antitrust laws. The major thesis of a rule of reason after all is that each case is unique. Secondly, in the interpretation of the Celler-Kefauver Act, as early as 1952 the Federal Trade Commission adopted the policy urged by Handler. The Department of Justice has been most circumspect in initiating proceedings involving mergers. Finally, the government assumes a very heavy burden in undertaking "a comprehensive analysis based on all relevant market data." A merger case becomes at least as broad as a Sherman Act case,<sup>4</sup> and the market effects are more difficult to assess, since they largely lie in the future. Rarely can the government count on willing witnesses from the trade. Excepting instances where a major supplier acquires one of their competitors, other firms generally welcome mergers. And where they fear the new consolidation, they may feel it poor business to parade their dangers before the world. As a result, the demonstration of probable competitive effects tends to rely heavily on statistical exhibits even though the merit of such evidence rarely is established without lengthy testimony. Thus the preparation and trial of a single merger case typically involves a substantial drain on the professional talent available to each of the parties involved.

FRANK J. KOTTKE

*Washington, D.C.*

<sup>4</sup> Perhaps broader, since whether the acquired corporation was a failing company frequently is an issue.

### **Land Economics; Agricultural Economics; Economic Geography; Housing**

*Urban Land Use Planning.* By F. STUART CHAPIN, JR. (New York: Harper and Brothers. 1957. Pp. xv, 397. \$8.00.)

Professor Chapin has not written a conventional textbook on urban planning. Rather, he has focused attention on one aspect of planning, namely, land use: the forces affecting the use of land, methods of analyzing such forces, and means by which planners can specifically plan land use. Within these limits it is by far the best book available, and since it is a study concerned primarily with the allocation of a scarce resource to alternative uses, there is much in the book of interest to economists.

In Part I Chapin very carefully and ably classifies the various forces controlling the use of land as economic determinants, socially rooted determinants, and the public interest as a determinant. In the process of classification, he supplies a census of the factors affecting the distribution of land among alternative uses through an examination and review of current literature and

practices. As a result, the section on determinants is as adequate as, and as inadequate as, all previous discussions of the same material. More importantly, the section lacks an integrating theme, and therefore there is no sense of the relationship among land uses. Chapin could have overcome this difficulty by centering discussion around the establishment of hypotheses concerning urban space arrangements or on the testing of hypotheses such as those of Homer Hoyt and Walter Firey. However, he has not chosen to do so and because of this lack of a central theme there is the implicit suggestion that all the determinants are of equal magnitude; yet there is evidence from other sources to indicate that they are not. There are, for example, many instances where the economic determinants of land use have forced major changes in public policies. Whether this is good or bad does not detract from the fact that it is erroneous to believe that all forces affecting the way in which land is used are of equal significance and it is important in any study to suggest the circumstances under which individual factors are likely to dominate.

Granting that economic forces, social forces, and the interests of the public are the determinants of land use, how do you measure these forces? In Part II, Chapin competently reviews various measurement methods. Considerable attention is devoted to the input-output analysis of Leontief as adapted by Isard; to Bogue's work on metropolitan regions; to the use of Census data for defining regions; and to economic base studies. This excellent review is followed by chapters on employment and population with emphasis on regional analysis. The final chapters in this section explore the relationship of transportation to urban land use—a most significant subject which is given only limited treatment—and methods of classifying, mapping, and reporting various types of land use.

In Part III, Chapin finally focuses attention on the key element in land-use planning—the relationship among different types of land use. Again, emphasis is placed on space and location requirements as reported in previous studies rather than on the development of principles or hypotheses. As a guide to the planner in terms of how to map, how much land to allocate for different uses, and so on, the section cannot be improved upon. However, Chapin does not point out to what extent the planner has freedom to select alternative patterns of possible land arrangements and to what extent his choice is limited by the determinants of land use. In the last analysis, if the planner cannot direct land use within fairly broad limits, then an understanding of the determinants of land use will always be sufficient for adequate planning, and there is little need for the development of a general theory of land allocation. By cataloguing the various factors affecting land-use arrangements rather than by analyzing the interrelationship of these forces and possible alternative land-use arrangements, Chapin does not come to grips with this problem.

The lack of attention to some of the fundamental issues of land planning does not, however, destroy the value of the work. Indeed, by emphasizing the necessity for economic and social analysis in urban planning, Chapin has made a very important contribution.

JAMES GILLIES

*University of California  
Los Angeles*



*The Federal Lands: Their Use and Management.* By MARION CLAWSON and BURNELL HELD. (Baltimore: Johns Hopkins Press, for Resources for the Future, Inc. 1957. Pp. xxi, 501. \$8.50.)

Within a generation the disposition of federal lands practically ended; this was followed by a wave of acquisitions, and there now has been reached a point of stability. Of incalculable noneconomic worth, the federal lands are of great direct and indirect economic value. Federal lands amounting to 24 per cent of the nation's total land area (377 million acres) produce \$331 million of annual gross receipts. This is roughly equal to the authors' estimated needs of those properties for development, and since 1950 there has been a positive balance of income over expenditures. The authors contend that the time has come for placing these active public assets on a more businesslike management basis. The book plainly declares that a much better job could be done.

Clawson and Held have written an introduction to a needed re-examination of the federal lands and the whole system of federal land management. Written and published under the sponsorship of Resources for the Future, a Ford Foundation subsidiary, the book is designed for a public generally interested in the federal lands and the issues concerning them. It will also be informative to those with special interest in aspects of federal land utilization, because the book considers the interrelations of the various public lands, their uses, and their management. Written in nontechnical language and with the authors' partisanship held in check, the book provides valuable instruction in the highly complicated maze of the federal lands, their administering agencies, and the large body of diverse legislation affecting federal land management.

After a simplified historical review of federal land disposition and custodianship, the authors give detailed consideration to the many uses of federal land, influences at work upon policies, and the utilization of the land. The senior author refers ruefully to the role of administrators, drawing from his own service as Director of the Bureau of Land Management. Perhaps the most interesting part of the book is the statistical material and graphic illustrations, both in the text and the appendix, which are designed to show the value of federal lands to the Treasury and to the national economy. It is a calm book free from propaganda but it will not end disputes over public land administration.

Setting as a goal the efficient utilization of resources, the authors plead for the adoption of new policies of management comparable to the efficiency of private development. They maintain that lessons from the past have relevance for present and future policy, and they suggest that those lessons point to a coupling of revenues and disbursements in a unified way under the supervision of a government corporation which would absorb the work of many scattered agencies. Detailed consideration is given the government corporation recommendation although alternate plans for reorganization are considered. The book provides a sound basis for discussing the public issues of federal land management.

JAMES H. SHIDELER

*University of California, Davis*



*The American Oasis.* By EDWARD HIGBEE. (New York: Alfred A. Knopf. 1957. Pp. xiv, 262. \$5.00.)

This book, a general treatment and written in a very readable style, will be interesting to the lay reader. Although some 21 general works on natural resources are included in a brief bibliography, neither footnotes nor specific citations of literature are included. This book is therefore not one that will prove particularly useful for purposes of research.

The foreword, by Fairfield Osborn, reflects that the work under review, "... describes, region by region, the varying characteristics of the productive lands of the United States, taking full cognizance of the geologic and climatic influences." In the preface, the author cites as a principal objective, "... the clarification of regional differences in our farming and related soil and water conservation problems." In a sense, the book satisfies both claims.

The first of the two main parts into which the book is divided gives an indication of the impact of different social cultures on land-use practices, including the impact of the various European immigrations on the native Indian land culture. The limitations of land use by virtue of its climatic and geologic characteristics are also touched upon. From the outset, the author's professional competence in agronomy is apparent. The early chapters present a lucid description of the main elements of soil formation, and discuss several types of climatic and biological influences that result in soils of differing agricultural potential.

The second part aims at a statement and explanation of current land uses and their historical evolution within six broad agricultural regions of the United States. Regional demarcation is drawn largely on the basis of the crop and farm types characteristic of portions of the country. In the discussion of regions, the most revealing comparisons are in terms of soil attributes.

In undertaking to deal with present farm types and tillage practices from the historical standpoint, little attention is devoted to the particular institutions that have been the vehicles of historical influence. Reference, in broad terms, is made to the Homestead Act, systems of water rights, and early New England land grants, but in no instance are the particular institutions analyzed in terms of their functional influence on land use.

Higbee's work is both prescriptive and descriptive, with frequent suggestions of desirable changes in land practices and use. These recommendations include increasing meadow lands and grazing culture, retirement of eroded lands, and other measures related to the physical properties of the soil. The fundamentally economic nature of issues associated with resource use and management is not brought out. Higbee recognizes certain present practices, but consideration of the causal interplay of economic and institutional factors that lie behind existing practices is largely omitted. Issues of taxation, credit, and producer's organizations have been omitted, and tenure is mentioned only briefly. The book is descriptive rather than analytical. The recommended changes in land use are predicated on physical criteria. In failing to deal with the institutional factors behind individual decisions on land use, the author's recommendations are general pronouncements, without indication as to methods of their implementation.

Agricultural and land-use policy cannot be formulated on the basis of physical data alone. It must be recognized that the individual user will make decisions on the basis of costs and returns. Depending on the economic context within which these decisions are made, the result may or may not be in accord with what the physical data indicate. Higbee states that, "... it is the potential productivity of land, not money, which is vital to society in the long run." It must be recognized that no less with society than with the individual farmer, the potential productivity of land must be valued, and monetary valuation has emerged as the most practicable method, although studies of uncertainty and time preference reveal that it is certainly not a perfect technique.

This criticism does not imply that the reviewer feels a statement of the physical characteristics of natural resources superfluous. Indeed, these are essential, but an analytical consideration of the social issues of resource-use within the physical context appears to be a prerequisite to policy planning. It is with this in mind that one can state the need for complementary work to that performed by Higbee before policy recommendations such as those appearing in *The American Oasis* can be validly put forward.

MICHAEL F. BREWER

*University of California, Berkeley*

*America's Natural Resources*. Edited for the Natural Resources Council of America by CHARLES H. CALLISON. (New York: Ronald Press. 1957. Pp. v, 211. \$3.75.)

This volume comprises essays or excerpts from other works by eleven authors and was sponsored by the Natural Resources Council of America. Seven chapters are devoted to particular resources, two to the general relationship between man and his environment, and one each to the statement of an ecological perspective for problems of the use of natural resources and to the need for and nature of a national resources policy. Each chapter ends with a brief list of suggested readings, but there are no other citations.

The seven chapters include a statement on soil by Firman Bear, which deals with selected soil conservation organizations, their history and current operations. Harold Wilm has contributed a chapter on water in which he indicates some of the economic and legal problems entailed in planning for and effecting water management. A chapter on grasslands by David Costello is perhaps the most informative contribution. The configuration of present vegetation is contrasted with the original grasslands of this country. Following a brief section describing selected grasslands uses, certain contemporary management issues are considered, including the use of fire and the grazing of federal lands.

A chapter on forests by Clepper and Besley includes a discussion of the history of the conservation movement within forestry, a brief mention of the size of forest industries in this country, and a statement regarding the contents of a needed national forestry policy. Joseph Shomon has contributed a chapter on wildlife, which gives a comparative picture of species and populations 150 years ago with those of today. It is suggested that the increasing pressure

of human population upon his fellow forms of wildlife will give rise to new measures of control on man's activities rather than the control of wildlife *per se*. He mentions the possibility of the zoning of land-use to prevent agricultural encroachment on wildlife habitat. A chapter on fish by Hazzard and Voigt focuses upon recreational aspects as opposed to commercial fishing and mentions the impact of certain water uses upon the fish population. In the final chapter on specific resources Harold Zahniser deals with parks and wilderness, presenting a chronological statement of the development of recreational parks and wilderness areas in this country. He then considers, in extremely broad terms, the general private and public benefits derived from them, concluding that the latter are sufficient to warrant specific legislative definition and approval of the wilderness concept rather than permitting it to remain the largely discretionary matter it is today.

Of the remaining four chapters, one by Shirley Allen briefly states the necessity for an ecological approach to the understanding of resource problems. Fairfield Osborn, in another, indicates the general types of problems concerning the use of renewable resources which have been brought about by world population increase. He notes that the conservation movement reflects an awareness of these problems, and that it constitutes the most promising influence upon the future status of our renewable resources. Edward Graham, in a chapter on land use principles and needs, makes four assertions that are reminiscent of natural laws: these define land, state two of its characteristics, and claim a relationship between human actions and natural environment. The concluding chapter by Ira Gabrielson considers the need for a natural resources policy. Little information is given as to the specific nature of such a policy nor the levels of state or national organization at which it would be effected.

This collection of pieces is interesting. It contains useful facts regarding some of our natural resources and several institutions concerned with their use. If its purpose is to point out problems and suggest answers, it may be criticized from several standpoints. The chapters on the individual resources fail to define specific problems of resource use or to identify the conflict of interests from which they arise. The more general chapters, while of a strongly normative character, fail to present any framework within which specific problems can be analyzed. In the absence of this, policy innovations or changes seem to be stated on an intuitive basis.

This volume, like many others, frequently employs the term "conservation," implying an inherently correct and good, albeit obscure, body of logic. If the specific problems to which it is addressed and the criteria adhered to in pursuing their solution are not stated, conservation can hardly be considered either consistent or logical. The term holds emotional and political implications, but until its area of concern and the criteria for decision are specified, it remains of questionable value in assisting the decision-making process.

MICHAEL F. BREWER

*University of California, Berkeley*

## Labor Economics

*The Economics of Discrimination.* By GARY S. BECKER. (Chicago: University of Chicago Press. 1957. Pp. x, 137. \$3.50.)

This is an unusual book; not only is it filled with ingenious theorizing, but the implications of the theory are boldly confronted with facts. Though the argument is at times quite technical, there is little attempt to engage in pyrotechnics for their own sake; the author wants theorems only in order to explain observed phenomena. The intimate relation of theory and observation has resulted in a book of great vitality on a subject whose interest and importance are obvious.

Becker begins his analysis by dividing what is received by an individual in a transaction into two parts; the receipt as ordinarily defined, and an element of satisfaction or dissatisfaction resulting from the social contact in which the transaction involves him. Where the contact required by a given class of transactions is considered a source of dissatisfaction, the net gain in such transactions is less than otherwise. In such cases, the market demand price will be lower than when there is no such dissatisfaction. If the contact is unpleasant to a specific individual, *and he behaves as though he were willing to pay to avoid such contact*, that individual is said to exhibit a taste for discrimination. (If the contact is pleasant, the individual is said to exhibit a taste for nepotism.) An individual's taste for discrimination (or nepotism) is reflected in a "discrimination coefficient," DC. This discrimination coefficient (with respect to a given type of transaction) is defined as a fraction of the money cost of that transaction.

Where an economically dominant section of a community exhibits a taste for discrimination against a certain class of individuals—say Negroes—in, say, the employment relation, the ratio of the wage rate for Negro labor services to that for comparable services rendered by whites will be less than in the absence of such taste for discrimination. The difference between the ratio of the wage rates for Negroes and whites in the presence of a taste for discrimination, and in its absence, is defined (p. 9, especially n. 6) as the "market discrimination coefficient," MDC. This coefficient is functionally related to the individual DC's, but it also depends upon the whole range of economic forces that affect the relative "economic importance" of various members of the community as this importance relates their individual DC's to the MDC.

Using these concepts, Becker proceeds to argue the following: (1) In a purely competitive society with two groups of persons, the effect of a taste for discrimination against one group (as reflected in a positive MDC against that group) is to reduce the per capita real incomes of both groups (pp. 11-13). (2) Discrimination will harm the group discriminated against (call it N) more than the discriminating group, W, "if N is more of an *economic* minority than W is a *numerical* minority" (pp. 18-19; my italics). Less abstractly, this means that if N furnishes less labor than W, discrimination will always reduce N's income relative to W's, but if N furnishes more labor than W, N will be

harmed by discrimination more than W only if the income yielded by all of N's resources (human and nonhuman) would be less, in the absence of discrimination, than the income yielded by all of W's resources. (3) When there is discrimination against N as labor sellers (but not to an important degree as employers) its effect is to raise the wage rate for W as laborers, but to harm W as capitalists by causing them to pay more than otherwise for their labor (pp. 13-14). (4) If the members of N attempt to retaliate, they will lower their own incomes further, and by more than they lower the incomes of the members of W (pp. 23-24).

The first two propositions are both correct and important; the last two are subject to some qualification. Proposition (3) is widely, though not universally, applicable.<sup>1</sup> I consider (4) to be misleading; it is true only in so far as it refers to small groups of persons. For if Negroes *collectively* refuse to deal with discriminators they may raise the marginal cost of discrimination sufficiently to alter the relative demand for their labor services and thereby increase their incomes.<sup>2</sup> The possibility of creating a seller's monopoly of Negro labor services and/or a monopsony of their household purchases does not contradict anything Becker says; his argument assumes pure competition. However, he neglects the fact that proposals for economic retaliation against discriminators almost always envisage collective action. It should, of course, be recognized that the success of collective action by Negroes would depend crucially upon whether collective counteraction was undertaken by whites.

One of the more important theoretical contributions of Becker's analysis is the sharp distinction drawn between discrimination and segregation (pp. 14-16, 48-50). He points out that the mere fact of white distaste for contact with Negroes need not, of itself, inflict economic injury upon the latter, provided Negroes do not have a parallel distaste for contact with one another. The existence of discriminatory tastes could result in Negro employers hiring Negro workers; selling to Negro customers, etc., leading to two identical economies, one Negro and one white. Economies of scale aside, this does not happen because whites have more complementary nonhuman resources per capita. Because of this, Negroes would earn less, under complete segregation,

<sup>1</sup> It is conceivable that N and W labor are strongly cooperant in production; i.e., the marginal productivity of either type of labor may increase with the amount of the other that is used (capital constant). In this case, discrimination by capitalists against either type of labor may lower the wage rates of both. A case where this might arise is where white labor is restricted to jobs of a supervisory character and requires Negro labor to supervise.

<sup>2</sup> To see this, consider the following possibility: Negroes initially offer white employers the following alternatives: (1) hire us on the same terms as whites or (2) none of us will work for you. If all white employers had a positive DC, none would (assuming competition) hire Negroes and whites on *identical* terms. But those with relatively small DC's would compromise on employment terms involving less discrimination than before. Employment of Negroes would then be more concentrated on employers with relatively low DC's, though there would be fewer Negroes employed. If the elasticity of demand for Negro labor (which depends, *inter alia*, upon the distribution of DC's) was less than one, their income would be increased.

than some of them could earn by working with white capital under discriminatory conditions; therefore, some of them accept discriminatory employment.

However, the pattern of discriminatory tastes is such that Negroes are compressed into job categories of low prestige and income. The relatively few cases of Negroes holding high-status jobs are disproportionately in government or in the segregated portion of the economy where Negroes supervise other Negroes, etc. Becker effectively uses the concept of economic segregation to explain the relative numbers of Negroes in various professions (pp. 71-73), and also why there is a negative association between value added per establishment and the relative number of Negroes employed.<sup>3</sup>

One of Becker's most challenging conclusions (Ch. 9) is that there has been relatively little change in discrimination during the interval 1910-1950. (He has data only for the years 1910, 1940 and 1950.) This conclusion is supported by a comparison of the relative "occupational position" of whites and Negroes in both the North and the South. "Occupational position" is measured by placing all workers in one of three occupational categories (skilled, semiskilled and unskilled) which are assigned numerical values proportional to the average incomes in 1939 of the white workers therein. Treating these numerical values as weights, and applying them to the percentages of workers (in each race) in each occupational category, a weighted average is constructed; this average indicates the occupational position of the racial group in the relevant year.

The relevant indices for both whites and Negroes climbed secularly during 1910-1950, in both North and South. However, in the North, the relative advance of the Negro index (as compared with that for whites) was small; and in the South, the Negro index declined relative to the white during 1910-1940 and, despite a slight relative advance during the 1940's, stood relatively lower in 1950 than in 1910. Consequently, Becker concludes (p. 114) "that almost all the increase in the absolute occupational position of Negroes was caused by forces increasing the position of whites as well. Changes in variables affecting the relative position of Negroes presumably either were minor or offset one another."

However, it is well known that since 1940 the relative median wage and salary earnings of Negroes rose appreciably as compared with those of whites,<sup>4</sup> which suggests that Becker's measure of relative occupational posi-

<sup>3</sup> Becker's argument on this point (pp. 69-71) may well be correct. However, I would feel happier about it if he had considered the following alternative: there is (1) a positive association between size of establishment and wage rates and (2) a negative association between establishment wage rates and the fraction of the work force that is Negro. These two associations together could produce Becker's result even though his hypothesis were false.

<sup>4</sup> H. M. Miller, *Income of the American People* (New York 1955), Table 51, p. 99. This table shows that in 1951, the median wage or salary income of nonwhites was 4.32 times its 1939 level, while the corresponding figure for whites was 3.01. (Similar relations hold for males and females taken separately.)



tion does not respond to certain types of forces which bear upon the relative earning power of Negroes. One of the more important of these forces is rural-urban migration, which differentially affects Negroes.<sup>5</sup> That is, new rural migrants to cities tend disproportionately to accept jobs at unskilled labor. Thus this type of occupational movement is likely to be undetected by Becker's index. But it would not be seriously denied that such migration reflects occupational improvement for the workers concerned.

Furthermore, because Becker's index uses fixed relative income weights, it fails to reflect the advance in income of unskilled occupations relative to that of others between 1940 and 1950. As Negroes are disproportionately represented in these occupations, the use of fixed-income weights tends to understate the relative economic improvement of Negroes.

A further weakness of Becker's argument on this point is that he presents his index for the South and the non-South separately, but not for the country as a whole. But there has been a secular movement out of the South by Negroes relative to whites, so that the relative nationwide occupational position of Negroes has improved, even though there may have been no change in relative position in either South or non-South separately. This type of relative improvement could take place even though every single individual DC remained unchanged. It may be this latter possibility to which Becker refers when he argues that there is little evidence that economic discrimination against Negroes has diminished.

Regional disequilibrium in the allocation of Negro labor is at least partially responsible for two other phenomena of which Becker speaks: (1) discrimination appears to be greater in the South than elsewhere (pp. 97-103); and (2) it is positively associated in urban areas with the relative number of Negroes (pp. 103-7). Becker "explains" this by asserting (pp. 86-87) that Negroes are more "mobile to the South,"<sup>6</sup> but he does not ask why Negroes do not move where discrimination is less. The tendency of Negroes to be relatively more numerous where their relative incomes are lower could be explained if one or more of the following were the case: (1) a tendency for Negro absolute income to be negatively correlated with the ratio of Negro to white income; (2) for some reason a preference existed among Negroes for those locations where their relative income was comparatively low and (3) a disproportionately large number of Negroes were born in areas where their relative incomes were low and, by migration, were in process of relocating themselves, but that the adjustment had not yet been completed.

<sup>5</sup> Thus the percentage of nonwhites who were farmers and farm managers or farm laborers and foremen shows a much greater decline between 1940 and 1950 than of the population as a whole. United States Census, 1950, Bull. PC-7 No. 2, Table 6, pp. 28-29.

	Total		Nonwhites	
	1940	1950	1940	1950
Farmers and Farm Managers	11.5	8.0	15.0	9.5
Farm Laborers and Foremen	4.3	2.8	11.0	3.9

<sup>6</sup> I.e., the supply curve of Negro labor services in the South lies below the curve referring to the North.



The first of these explanations is inconsistent with well-known facts and the second seems highly implausible, which leaves the third.

The validity of this last explanation follows from the fact that Negroes are known to be concentrated in certain occupational categories (*i.e.*, non-skilled labor) which tends to increase the relative supply to these occupations, and lower their relative wages. Thus, if labor demand and white labor supply were the same in two locations, that location which had the greater proportion of Negroes would have relatively lower wages in those occupational categories in which Negroes were concentrated and the relative income of Negroes would also be lower.<sup>7</sup> Intercity and regional differences in the ratio of Negro to white incomes might thus be explained without assuming the existence of any discrimination. Since, however, it is quite unlikely that Negroes and whites in the same occupations in given communities have the same incomes (the differential incidence of unemployment would almost preclude it), there must be another factor or factors at work. Discrimination is an obvious candidate, and Becker is right in emphasizing it. However, it seems likely that the influence of labor market disequilibrium, which Becker ignores, also plays an important role in explaining these phenomena.

There are several phenomena to which Becker calls attention that require a more detailed theory about the taste for discrimination than anything so far mentioned. One of these is the fact that the relative median income of Negroes tends to decline as we move from persons with less to those with more years of schooling. Becker's rather tentative, though plausible, explanation of this fact is that the aversion of whites to contact with Negroes is greater the higher the occupational status of the Negro (pp. 124-25).

A lesser aversion to Negroes in temporary than in permanent jobs is posited to explain why the ratio of Negro to white mean income was lower, and the relative number of Negroes less, among persons employed 12 months (in 1939) than among those employed less than 12 months (pp. 86-87). These alleged differences in tastes for discrimination seem consistent with what is generally known. However, the difference between the Negro-white mean income differential for persons employed 12 months and that for others is open to an alternative interpretation. Among the persons employed less than 12 months are included not only those who seek work steadily but also seasonal workers and others out of the labor force during part of the year. The differential incidence of unemployment upon Negroes placed a larger fraction of full-time Negro job seekers in the "less than 12 month" class, thereby creating an upward bias in the ratio of Negro to white mean incomes in this category relative to its complement.

The critical tenor of some of the above comments should not be interpreted as reflecting in any way on the general quality of Becker's empirical work; on the contrary, this work is both skillfully and carefully done. As a

<sup>7</sup> This hypothesis implies (*ceteris paribus*) that the differential for skilled over non-skilled workers will be greater, the greater the proportion of Negroes in the labor force. This is consistent with the gross facts of experience, but it should be tested in detail. This hypothesis, as far as I can see, cannot be derived from Becker's model.

result this is a vital book that will influence thinking on the economics of discrimination for a long time to come.

M. W. REDER

*Stanford University  
and Center for Advanced  
Study in the Behavioral Sciences*

*Contemporary Collective Bargaining in Seven Countries.* Edited by ADOLPH STURMTHAL. (Ithaca: New York State School of Industrial and Labor Relations, Cornell University. 1957. Pp. ix, 382. \$4.50.)

This is the fourth in a series of studies on international labor problems. It presents the collective bargaining systems of seven Western nations and concludes with the editor's analysis of uniform and diversive trends. Sturmthal assembled an impressive panel of experts and the countries selected serve to demonstrate the many ways and means by which collective bargaining, with its "annex" of government regulations, achieves its ends under widely different institutional patterns.

First, Allan Flanders summarizes the postwar Labor market in Great Britain as "most remarkable for its lack of innovations." He points out that interim war trends largely continued after 1945. By 1950, collective wage determination covered some four-fifths of all employees but its methods and procedures varied from industry to industry and consisted roughly of three types: collective bargaining, bargaining through joint industrial councils, and statutory wage-setting by wage boards. The methods often overlap, however, and are, as Flanders indicates, not so essentially different since statutory wage-setting also follows bargaining procedures. Flanders devotes much of his essay to comparisons of the British situation with continental and American labor markets and finds many significant contrasts that apply, in particular, to the concepts of a national wage policy.

John Inman's report on the Norwegian system of collective bargaining contrasts strongly with the British situation. He explains that the structural development of union and employers' federations provided optimum conditions for a centralized wage policy. Moreover, the control of these associations over their affiliates made it possible for the national wage policy soon to be shifted from government arbitration to a system of private compulsion based on the national federations of workers and employers. Inman analyzes the content of this wage policy with regard to the wage structure and to general wage levels; he reports that union wage restraint assumed at times the form of complete wage stops but indicates that "unofficial" increments of earnings tended to counteract the official wage policy. In spite of its shortcomings and of the inflationary impact before and since the Korean War, the author feels that Norwegian wage policies have more or less achieved their objectives.

Similar to that of Norway, the collective bargaining system in the Netherlands after 1945 was characterized by a high degree of centralization, backed up by the collaboration of its multiple structure of ideological labor and employers' federations. P. S. Pels offers a vivid picture of the objectives, as well as

of the problems of a wage policy on the tripartite basis of organized labor, organized management, and a high degree of government participation. The objectives of this truly national policy were partly given by the needs of reconstruction, partly by the workers' willingness to put employment before wages and, finally, by the desire for "social justice" which implemented meant a planned and rational system of permissible wage differentials. Inman's report on the gradual emergence of such a policy, based on classified skill differentials, and on a national system of job evaluations, is particularly interesting. Yet recent developments indicate that, even in the Netherlands, a second thought seems to question the advisability of too narrow differentials. It seems that, under the tight regime of wage controls, price inflation has been more limited than in other countries, but that real wages have also lagged during part of the period.

Sturmthal's study of the French labor market eliminates all notions as to a more or less "uniform" Continental bargaining system. The author traces the development of collective bargaining conditions in France back to 1919, and points to the apparent weakness of unionism during most of the period as the main reason that government regulations were needed to supplement the actual bargaining strength of the divided, and not too bargaining-conscious, labor movement. Up to 1950, Sturmthal reports, French wages rested largely on public regulations motivated by anti-inflationary considerations. The more recent emphasis on freer collective bargaining leaves the government still with a strong influence.

Clark Kerr examines the collective bargaining system of Western Germany and finds that it shows "few obvious traces" of the past Nazi period. Two important differences separate it from Weimar: the trade union movement has been unified, and compulsory public arbitration has been abolished. Kerr's analysis of postwar policies explores the attitudes of the unions and employers' associations and their relative strength. He finds that organized labor in Germany has made a "choice of political over industrial warfare" (the struggle for codetermination); that members prefer employment security over strikes and aggressive wage policies; that employers' associations are more disciplined than unions; and that the climate of negotiation is still largely that of a "class society": tense and formal. The German wage structure appears to the author more compressed than the American.

Two chapters of the study deal with postwar Italy. Luisa Sanseveroni traces the evolution of collective bargaining from its Fascist heritage to the middle fifties. The postwar emphasis, the writer reports, was on voluntarism; government regulations pertained mainly to the constitutional provisions (such as the certification of unions or the institution of works councils dealing with grievances, layoffs and other issues), and to a policy of making collective bargaining more effective. Sanseverino outlines the complex structure of bargaining ("sector-wide" and "industry-wide") and the superstructure of wage payments in Italy. On the latter subject, Cesare Vanutelli provides a more detailed analysis. Summarizing his findings, it can be said that cost-of-living allowances and social charges tended to make basic wage rates (based on skills, geographic

regions, etc.) less and less important. A more recent agreement instituted a readjustment allowance in favor of the higher-paid skilled labor groups.

Neil W. Chamberlain provides a comprehensive picture of the American labor market as it has emerged from the secular growth of unionism. He points to the importance of union locals in the American system of collective bargaining; his essay contains a brief survey of the legal and institutional framework of American labor markets and analyzes the wage patterns of postwar bargaining. He reviews the discussions among economists of the impact of union wage policies on prices under full employment.

In Sturmtal's concluding essay on comparative collective bargaining the share of governments as a third party in labor-market policies is evaluated with regard to the diversified conditions of the countries discussed. The author also examines the trends of types of wage differentials and the causes behind their more or less pronounced reduction in various countries. The concept of a "rational wage structure" is examined with regard to its economic implications. Finally, Sturmtal deals with restrained wage policies of unions under full employment, and offers his conclusions for their effectiveness under short-run and long-run conditions of full employment.

The reader will find that the editor and his collaborators have made a notable and significant contribution to the literature of comparative collective bargaining, and, likewise, to the contemporary discussions of wage policies under full employment.

LUDWIG WAGNER

*Duquesne University*

*The Teamsters Union.* By ROBERT D. LEITER. (New York: Bookman Associates. 1957. Pp. 304. \$5.00.)

One should be forewarned that this book does not deal with "the shame of a union." It is, instead, in the words of the subtitle, concerned with "a study of its economic impact." Professor Leiter admits that most of his research was completed prior to the recent revelations about the Teamsters Union. But even if this were not so, it still would be the type of book that should have been written. For only through an understanding of the economics of the industry, with which the writer concerns himself in part, can one fully understand the present position of the Teamsters Union.

Leiter gives us not only a general history of the Union, but also a picture of its leadership and the various problems that it has faced over the years. He describes the industry as consisting of a large number of small-scale operators who require small amounts of capital in order to start in the trucking business. As a result there is no problem of entry into the industry. In turn, one finds "truck operators poorly trained, inadequately financed, and in some cases irresponsible."

It seems to the reviewer that this short and incomplete summary of the economics of the industry explains a good deal about the Teamsters Union. Although Leiter does not always make the specific connection, many of the problems the union has faced and most of the methods it has employed for

their solution are easily understood when related to the economics of the industry. Let me illustrate. The existence of a large number of small operators automatically makes organization and bargaining difficult. Therefore, the Teamsters Union helped organize employer associations with whom they could negotiate and obviously felt it necessary to obtain the closed shop as a basis for organizing the workers in the industry. Thus, the alleged close connections between the union and employers are quite understandable.

Another illustration: the strength of the union, as Leiter points out, stems from the fact that truck operators are fearful that if a strike took place shippers would provide their own means of transportation. Thus, truck operators are extremely vulnerable. In view of the fact, also, that certain industries have established their own shipping facilities it is to be expected that jurisdictional disputes would develop between the Teamsters Union and other industrial unions. And Leiter devotes a chapter to this subject.

If one accepts the hypothesis that an understanding of the economics of the industry will yield insights into the labor relations problems of that industry, then the book under review can be exceedingly useful as a case study. It seems to the reviewer that such an approach leads to an understanding of the problems found not only in the Teamsters Union but other unions as well. If this hypothesis is acceptable then one might well question whether the installation of "good" men in unions or the enactment of legislation will necessarily eliminate some of the activities of unions which are currently being revealed by Congressional committees.

What is commendable about the book is that it gives us a basis for understanding why certain things have occurred in the Teamsters Union. This does not mean that we condone such activities. But unless one understands the basic causes it is impossible to offer appropriate solutions.

JACOB J. KAUFMAN

*The Pennsylvania State University*

*Labor in a Growing Economy.* By MELVIN W. REDER. (New York: John Wiley & Sons. 1957. Pp. xii, 534. \$6.50.)

In assessing the merits of a textbook for review purposes one is usually forced to adopt a number of yardsticks—none of which is satisfactorily objective. Judged by this reviewer's criteria of appeal to students, comparison of presentation and content with alternative labor texts, and "the kind of textbook I should like to use," Professor Reder's addition to an already abundantly stocked field is a 2½-star entry. (In fairness, it should be added, this reviewer's "ideal" 3-star textbook would probably be a publisher's failure.) Apart from a surfeit of parenthetical comments, Reder's style is fluid and easy, making reading a pleasure rather than a chore. The chapters flow smoothly into one another so that one feels that the book comprises an integrated whole. The content and order of presentation are rather traditional, but competently—and in some areas, superlatively—handled.

The book is divided into two major parts, an eight-chapter section concerned with "Unions and Collective Bargaining," followed by seven chapters

on "Employment, Wages, and Income." While this is roughly a division into "institutional" and "theoretical" material, Reder states (p. viii):

It has been my endeavor, in all chapters of the book, to interweave theoretical and descriptive materials so that they are inseparable. If the book could be said to have a motto, it would be: "No theories without facts, no descriptions without explanations."

This aim is well conceived and well executed.

The institutional section opens with two chapters on the growth of American unionism, emphasizing organization and philosophy. These are good concise surveys, including an analysis of the causes and possible consequences of the AFL-CIO merger. Succeeding chapters on the organizational structure of trade unions and the process of collective bargaining are among the best in the book. Between the lines one can see the shadow of modern organizational, decision-making, and games theory, although the material is presented on a level well suited to the undergraduate. (In fact, the student will not even realize that he has been subjected to theory.) Part II is rounded out by a chapter on the content of collective bargaining, and three chapters on the relation of unions and government. In the latter section major emphasis is placed upon the reasons for enactment, and the consequences, of the Taft-Hartley Act. This is a very well-balanced treatment for the student who approaches the subject with strong biases of either extreme. Throughout this whole section Reder skilfully concentrates on the salient aspects of unionism and industrial relations without getting bogged down in a lot of interesting but irrelevant minutiae.

"The chapters on wage theory were, by far, the most difficult to write," notes Reder in the preface; they are also the most difficult for a reviewer to assess because of the very marked differences in taste of potential instructors. The first three chapters of Part III give a theory of wage determination without reliance on traditional concepts (*e.g.*, marginal productivity) and without diagrammatic treatment. These are followed by an excellent discussion of the structure of wages, a chapter on the formal procedures of wage-setting in the firm, and two chapters devoted to the macroeconomic aspects of wages, income and employment. The theoretical discussion is carried on at a rather low level of abstraction. The instructor who wishes to involve his students in a minimum of economic theory can follow the text closely; a good theoretical structure lies behind the rather simplified presentation, and the student is bound to absorb some of it. Alternatively, the instructor who wishes to emphasize labor *economics* can easily supplement the text without being undermined by it. The chapters on wage determination represent a compromise, and although this involves the author in some constraints which may seem unnecessary to many users of the book, it is carried off quite successfully.

The book concludes with three general chapters on causes of income inequality and public and private measures for guaranteeing minimum income standards.

The reservations the reviewer has concerning the theoretical treatment are of a passive nature. Reder's chapters on wage theory are as good as those of



two or three other textbooks, and much better than most. When a good theorist sits down to write an undergraduate textbook, it is cause for rejoicing; it is only unfortunate that some publisher will not make way for a book more particularly aimed at the undergraduate with a modicum of theoretical sophistication.

Judged by the standard of alternative books in this field, Reder has written an undergraduate labor textbook which, in style, presentation and content, must rank with the two or three best ones currently available.

ALLAN CARTIER

*Duke University*

*Labor Economics.* By PAUL SULTAN. (New York: Henry Holt & Co. 1957. Pp. xii, 580. \$6.50.)

This is an unusual labor economics text. For an economics text, unusual in the extent to which material from other disciplines, such as social psychology and anthropology, are woven into the economic description and analysis. For an introductory text, unusual in the depth of discussion of many issues in organization and collective bargaining. But unusual, too, in the relatively brief treatment of theory (about 80 pages).

Sultan's approach throughout the book is from the side of change and development. In every section there is either an historical development of the subject or heavy historical reference. Some readers may find the historical hand too heavy. But in most cases the historical presentation is such as to induce appreciation of changes in philosophy as well as practices. No attentive student will miss the historical contrasts or the bases of present institutions and ideas.

Some idea of the emphasis on dynamics is given by the titles of the five sections of the book: The Impact of Economic Change and the Evolving Labor Market; Unionism: Determinants and Characteristics of Growth; The Dynamics of Collective Bargaining: The Process and Its Impact; The Public Interest and Industrial Relations: A Review of Labor Legislation; and The Economics of Wages and Employment.

Typically, the discussion of wage theory is heavily historical. After a thorough discussion of mercantilist, classical, and marginal productivity theory, the bogey of unemployment is thrown in. Thus the reader is caught up in the dissatisfactions of twentieth-century economists, Keynesian analysis, and current problems of wage policy. This presentation is novel, for a text, both in plan and execution, and on the whole is effective.

The book's greatest weakness is imbalance and overdetail in the treatment of some issues and institutions. Sultan seems to go to extremes in presenting full description and analysis of certain topics, e.g., labor racketeering, rank-and-file participation, control by the national hierarchy. It is difficult to see how some of this detail will benefit or arouse the interest of the undergraduate beginning the study of labor economics.

Teachers will find in this text much quotation from the source materials for which they have been using books of readings. In addition, the book is heavily documented throughout. There are more footnotes than this reviewer



has seen for many a day in an introductory text. In some of these notes are interesting discussions and quotations, which the student should not miss. (But most students will!) A very annoying practice is the frequent use of second-hand citations accompanying quotations—citations not to the original sources, but to other authors who quoted. There are at least sixty such citations, most of them to easily available originals. Either Sultan is scrupulous in identifying specifically his obligations to other authors for finding good references, or he is negligent in not, himself, going back to the originals.

Sultan's greatest successes are in the breadth of his treatment and the emphasis on dynamics—both of which go well beyond other texts.

HOWARD M. TEAF, JR.

*Haverford College*

### **Population; Welfare Programs; Standards of Living**

*The Economic Status of the Aged.* By PETER O. STEINER and ROBERT DORFMAN. (Berkeley and Los Angeles: University of California Press. 1957. Pp. xx, 296. \$5.00.)

Students of social policy in regard to the aged have good reason to be grateful to Professors Steiner and Dorfman. An impressive body of data concerning the labor force and income status of the population aged 65 and over has been made available, based on a special nationwide follow-up study carried out by the Census Bureau at their request in connection with the Current Population Survey of April 1952. Hitherto, our most comprehensive information about the economic status of the aged had been based on the Survey of Old Age and Survivors Insurance Beneficiaries (the so-called "Beneficiary Survey") which was conducted by the Social Security Administration in 1951. Comprehensive as was the information sought in this survey (indeed in some respects it covered more ground than the Steiner-Dorfman study), the group was a selected one and, being limited to beneficiaries, necessarily excluded uncovered persons (a fairly large group in 1951) and aged persons under 65 who had not ceased working or who had benefits withheld under the then relatively low earnings limit.

The data collected in the follow-up study has been analyzed and organized by the authors with scrupulous care, and the value of the book is greatly enhanced by five methodological appendices. It covers the social characteristics of the aged, the labor-force status of men and women, their receipts and needs, their support status, the sources of their receipts and the size and adequacy of their assets.

Some of their findings are perhaps not as novel as the authors imply. The fact that, for example, the aged are not a homogenous group, or that the major problem group consists, and will increasingly consist, of aged women and notably widows will come as no surprise to students of social security policies. In other respects their findings lend support to hypotheses drawn from general observation. Thus it is being increasingly recognized that the social problem of "the aged" is in significant measure a problem of the broken family: the

aged individuals who present the greatest need for social action, whether in the form of socially provided income, housing, or medical care and hospitalization, seem to be the single individuals and the widowed. It is thus of particular interest that the authors find that there is a significant relationship between the presence of a wife and the fact that an older man feels "well enough to work" and that if he is living with his wife he is more likely than other older men to be in the labor force, a difference that persists even when identical age groups of men are compared.

In view of the emphasis currently placed on the desirability of policies and programs directed to overcoming the prejudice against employment of older workers and on special guidance and placement services for the aged, perhaps the most interesting of the findings concerns the reasons for non-labor-force participation on the part of men. In 1952, 59 per cent of men 65 and over were not working. Of this group 24 per cent (13 per cent of all aged men) had ceased work involuntarily because of the provisions of a retirement system, age, lay-off or other reasons. But another 60 per cent (33 per cent of all aged men) were not in the labor force because of poor health. Curiously enough, the authors classify "health" as a "voluntary" reason for retirement, a classification which may well give rise to much misunderstanding unless care is taken in interpreting this table (p. 48) which is likely to be given much publicity. On the basis of these findings the authors hold that efforts to increase the employment opportunities of those who are already old are necessarily limited in effectiveness: "What is required is attention to prevention rather than to rehabilitation. This, in turn, implies a shift of focus to younger men" (p. 48). They take, however, a dim view of the impact of medical advances on the economic problems of older people, partly because they anticipate that these may increase not only working life but also life expectancy. They appear to underestimate the probable effect on ability to work of more adequate preventive and curative health services throughout life prior to age 65. Not all those who reported that they "did not feel well enough to work" were suffering from degenerative or chronic age-related diseases.

The authors also expect little success from efforts to mitigate the influence of occupational obsolescence, which they find to be a second major cause of withdrawal from the labor force (frequently in combination with health). Holding that in the future, as in the present, large numbers of the aged will be unable to provide satisfactorily for themselves, they conclude that the comprehensiveness and generosity of programs of public or private support will be a major factor in determining the economic status of the aged.

Yet the present aged are the product of a time when education was much less freely available than it now is, when wages were lower, living conditions less healthy and social services far less highly developed. Thus in planning for the future we must expect that the social, health, occupational and income characteristics of each ten-year cohort will show significant differences from preceding cohorts. It is probably too much to expect that the authors can be persuaded to undertake the time-consuming task of repeating their

survey ten and twenty years from now, but it is to be hoped that surveys such as theirs will continue to be made at regular intervals lest social policy in the future, as so often in the past, be based on out-of-date information.

The book is not easy reading, in part because of the extreme care taken by the authors to test the validity of their findings and of such relationships as they establish. It is, however, an indispensable document for all those who are concerned about one of our major social problems.

EVELINE M. BURNS

*New York School of Social Work,  
Columbia University*

# TITLES OF NEW BOOKS

## General Economics; Methodology

- BERLE, A. A., JR. *Economic power and the free society*. (New York: Fund for the Republic. 1957. Pp. 20.)
- BOULDING, K. E. *Principles of economic policy*. (Englewood Cliffs, N. J.: Prentice-Hall. 1958. Pp. viii, 440. \$7.95.)
- . *The skills of the economist*. (Cleveland: Howard Allen, Toronto: Clarke, Irwin. 1958. Pp. vi, 193. \$3.50.)
- EISERMANN, G. *Wirtschaftstheorie und Soziologie*. (Tubingen. J. C. B. Mohr (Paul Siebeck). 1957. Pp. 26. DM 1,90.)
- GATES, T. R., ed. *The economic almanac 1958—a handbook of useful facts about business, labor and government in the United States and other areas*. (New York: Thomas Y. Crowell, for Nat. Indus. Conference Board. 1958. Pp. xiii, 673. \$3.75.)
- HOMAN, P. T., HART, A. G. AND SAMETZ, A. W. *The economic order—an introduction to theory and policy*. (New York: Harcourt, Brace. 1958. Pp. xxiv, 829. \$6.95.)
- PAPANDREOU, A. G. *Economics as a science*. (Philadelphia: Lippincott. 1958. Pp. xi, 148.)
- PEACOCK, A. T., TURVEY, R., STOLPER, W. F., HENDERSON, E., ed. *International economic papers no. 7*. Translations prepared for the International Economic Association. (New York and London: Macmillan. 1957. Pp. 184. \$3.50.)
- STONIER, A. W. AND HAGUE, D. C. *A textbook of economic theory*. 2nd ed. (New York: Longmans, Green. 1957. Pp. x, 513. \$5.)
- WAFFENSCHMIDT, W. G. *Wirtschaftsmechanik*. (Stuttgart: W. Kohlhammer. 1957.)
- United States economic problems in the next twenty years*. Fifty distinguished scholars and leaders in public affairs present their views. (New York: Com. for Econ. Development. 1957.)

## Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

- BENTZEL, R. *Den privata konsumtionen i Sverige 1931-65*. (Stockholm: Industriens Utredningsinstitut. 1957. Pp. xvi, 475.)
- This book is a modest counterpart to Stone's studies of consumption in Britain. On the basis of consumption expenditures in the period 1931-1955, conventional regressions have been computed with electronic computing machines. The chief purpose was to produce a forecast for 1965. There is some discussion of consumption theory.
- BOMBACH, G. AND GABRIEL, S. L. *Löhne und Preise*. (Darmstadt: C. W. Leske. 1957.)
- BORCHERS, H. *Das Abstraktionsproblem bei David Ricardo*. (Jena: Gustav Fischer. 1929.)
- BÖSSMANN, E. *Probleme einer dynamischen Theorie der Konsumfunktion*. (Berlin: Duncker & Humboldt. 1957.)
- CANNAN, E., ed. *Lectures on justice, police, revenue and arms—delivered in the University of Glasgow by Adam Smith, reported by a student in 1763*. Reprints of Econ. Classics. (New York: Kelley & Millman. 1956. Pp. xxxix, 293. \$7.50.)
- CROSARA, A. A. *Il guidizio economico, la proprietà e il tempo nella questione sociale (capitale, interesse e ripartizione dei consumi)*. 2nd ed. (Rome: Ed. Studium. 1957. Pp. viii, 235. L. 2000.)
- DORFMAN, R. SAMUELSON, P. A. AND SOLOW, R. M. *Linear programming and economic analysis*. (New York: McGraw-Hill. 1958. Pp. ix, 527. \$10.)
- FÖRSTNER, K. AND HENN, R. *Dynamische Produktionstheorie und Lineare Programmierung*. (Meisenheim/Glan: Anton Hain. 1957. Pp. 125. DM 14,50.)

- HENDERSON, H. *Supply and demand*. Cambridge Econ. Handbooks reprint, originally pub. 1921. (Chicago: Univ. of Chicago Press. 1958. Pp. xi, 142. \$2.25.)
- JONES, R. *An essay on the distribution of wealth, and on the sources of taxation*. Reprints of Econ. Classics. (New York: Kelley & Millman. 1956. Pp. xlix, 378. \$8.50.)
- KENNEDY, W. F. *Humanist versus economist—the economic thought of Samuel Taylor Coleridge*. Univ. of California Pub. in econ. vol. xvii. (Berkeley: Univ. of California Press. 1958. Pp. 96. \$2.)
- LIPSCHITZ, E. *Die theoretischen Grundlagen David Ricardos in Lichte des Briefwechsels*. (Berlin: Duncker & Humblot. 1957. Pp. 214. DM 18.)
- ROBBINS, L. *Robert Torrens and the evolution of classical economics*. (New York: St. Martin's. London: Macmillan. 1958. Pp. xiii, 367. \$7.50.)
- ROBERTSON, D. H. *Lectures on economic principles*. Vol. 1. (London: Staples. New York: John de Graff, distrib. 1957. Pp. 172. \$3.50.)
- SHACKLE, G. L. S. *Time in economics*. F. de Vries lectures delivered in Amsterdam in 1957. (Amsterdam: North-Holland. 1958. Pp. 111. f 6.50.)
- The lectures are concerned with the following aspects of the general subject: (1) the complex concept of economic time; (2) decision and uncertainty; (3) some dynamic mechanisms (including investment, the multiplier, and the rate of interest). There is an appendix on the limitations of economic theory.
- SURIYAKUMARAN, C. *The economics of full employment in agricultural countries, with special reference to India and Ceylon*. (Kandy, Ceylon: K. V. G. de Silva. 1957. Pp. 307. Rs 12/50.)
- SYLOS LABINI, P. *Oligopolio e progresso tecnico*. Pub. della Facoltà de Econ. e Commercio del Univ. di Roma no. 5. (Milan: A. Guiffre. 1957. Pp. 206. L. 1000.)
- VEBLEN, T. *The theory of business enterprise*. A Mentor Book. (New York: New Am. Lib. 1958. Pp. 223. 50¢.)
- VON MISES, L. *Theory and history—an interpretation of social and economic evolution*. (New Haven: Yale Univ. Press. 1957. Pp. ix, 384. \$6.)
- WEINTRAUB, S. *An approach to the theory of income distribution*. (Philadelphia: Chilton. 1958. Pp. x, 214. \$6.50.)
- WICKSELL, K. *Select papers on economic theory*. Edited and with an introduction by Erik Lindahl. (Cambridge: Harvard Univ. Press. 1958. Pp. 292. \$6.50.)
- The articles included have not been previously published in English. "The papers have been divided into four groups. The first contains two early lectures, one explaining Wicksell's views on economics in general, the other giving a summary of his monetary theory. The second group consists of three papers containing the kernel of Wicksell's contributions to the theory of production and distribution. . . . Some articles . . . on the works of other well-known economists of his time have been put together as a third group. . . . The last group contains some papers on foreign trade problems, written by Wicksell after the end of the First World War." (From the editor's preface.)
- Mechanics of inflation—an analysis of cost and demand pressures on the price level*. (Washington: U. S. Chamber of Commerce. 1957. Pp. 69.)

### Economic History; Economic Development; National Economies

- BOVILL, E. W. *The golden trade of the Moors*. (New York: Oxford Univ. Press. 1958. Pp. ix, 281. \$7.)
- "There seemed to me a need for a book showing how the Sahara enriched the Carthaginian and bewildered the Roman; how in later times the greater caravan routes, linking the sophisticated cities of the north with the great markets and modest seats of learning of the south, not only influenced the course of events in Barbary, and even beyond, but sometimes determined it; how, all down the centuries, Berbers and Arabs, Jews and Christians, never ceased to draw on the wealth and industry of the Sudanese." (From the preface.)

COLM, G. AND GEIGER, T., assisted by HELZNER, M. *The economy of the American people—progress, problems, prospects.* Planning pamph. no. 102. (Washington: Nat. Planning Assoc. 1958. Pp. viii, 167. \$2.)

This little volume is a staff report of the National Planning Association. The scope of the report, which is designed to be broadly informative for the general reader, both in this country and in foreign countries, who seeks to understand the functioning of our economy, is clearly indicated in the title of the pamphlet. Part I explains how the American economy achieves its great productivity and high living standards, and Part II directs attention to its more important prevailing problems and general future prospects. In the progress aspect of the analysis, the operative factors which are examined include natural resources, labor, business management, research and technology, capital, government, and the values and institutional arrangements which have conditioned these factors and have rendered them outstandingly fruitful. In the analysis of the crucial problems which have fashioned its changing character, the examination includes the issue of balance in economic growth in relation to price stability and full employment, of advances in living standards in relation to the distribution of income, of concentration in economic power in relation to the maintenance of free competition and democratic ideals, and of international economic programs and policies in relation to the needs of underdeveloped countries and their political as well as economic significance. The concluding chapter, dealing with the nature and outlook of the American economy, emphasizes the fact that it "does not operate in accord with any of the 'pure' laws of laissez faire capitalism or of socialism," that it has succeeded in reconciling "the Hamiltonian idea of economic progress with the Jeffersonian ideal of individual self-reliance," and that if war can be avoided, "the United States has the possibility of achieving material abundance for all within the next decade or two." This exposition, largely confined to the actual accomplishments of the economy, is supported by simple but helpful charts; and an appendix contains, for convenient reference, a useful group of relevant statistical tables. The National Planning Association has rendered an important service in providing an excellent brief analysis and appraisal of the American economy.

I.L.S.

DIA, M. *L'économie Africaine—études et problèmes nouveaux.* (Paris: Presses Univ. de France. 1957. Pp. viii, 119. 500 fr.)

ENGELS, F. *The condition of the working class in England.* Transl. and edited by W. O. Henderson and W. H. Chaloner. A new translation based on the original German first edition in 1845. (New York: Macmillan. 1958. Pp. xxxi, 386. \$5.)

ERHARD, L. *Prosperity through competition.* Transl. and edited by E. T. Roberts and J. B. Wood. (New York: Praeger. 1958. Pp. xii, 260. \$5.)

FOTTI, V., ed. *L'automazione e le sue conseguenze sociali.* Studi e ricerche I. (Torino: Politica e Società. 1957. Pp. viii, 247. L. 1500.)

FOURASTIE, J. AND LALEUF, A. *Révolution à l'Ouest.* (Paris: Presses Univ. de France. 1957. Pp. 236. 600 fr.)

GHOSH, A. *Indian economy—its nature and problems (a new look Indian economics).* (Calcutta: World Press Private. 1957. Pp. xvi, 368. Rs 8.50; 16s.)

Designed as a text for college and university courses in Indian economics, "... the analysis in this book is primarily oriented toward growth problems. It poses both the old and the new problems of our economy from an angle which focusses the spotlight on economics of development. Its main purpose is to show how the Indian economy is being transformed under the impact of developmental planning." (From the preface.)

GHOSH, O. K. *Problems of economic planning in India.* (Allahabad: Kitabistan. 1957. Pp. viii, 159. Rs 5.50.)

HERNANDEZ SEGURA, R. E. *La planeacion y el desarrollo economico de El Salvador.* (San Salvador: Ministerio de Econ. 1957. Pp. 85.)

HUPPERT, W. *Gesetzmässigkeit und Voraussehbarkeit des wirtschaftlichen Wachstums.* (Berlin: Duncker & Humblot. 1957.)

HUSAIN, A. F. A. *Human and social impact of technological change in Pakistan—a report*

- on a survey conducted by the University of Dacca and published with the assistance of UNESCO. 2 vols. (Dacca: Geoffrey Cumberlege, Oxford Univ. Press. 1957. Pp. xix, 404; viii, 344. \$2.65.)
- JAIN, P. C. *Problems in Indian economics*. 3rd ed. (Allahabad: Chaitanya Pub. House. 1956. Pp. viii, 688. Rs 11/8; 25s; \$3.50.)
- JEANNENEY, J. M. *Tableaux statistiques relatifs à l'économie française et l'économie mondiale*. Cahiers de Fond. Nat. des Sci. Pol. no. 87. (Paris: A. Colin. 1957. Pp. 204. 1.000 fr.)
- JEANNENEY, J. M. AND PERROT, M. *Textes de droit économique et social français 1789-1957*. Cahiers de Fond. Nat. des Sci. Pol. 89. (Paris: A. Colin. 1957. Pp. xix, 711, 2.900 fr.)
- The texts of more than 500 pieces of important legislation, of significance in the economic and social history of France, have been assembled in chronological order. The first one is the decree of August 11, 1789 abolishing the feudal regime; the last one with a few minor exceptions, is the law of August 2, 1957, authorizing the President to ratify the treaty instituting the European economic community and Euratom. Each of the texts is preceded by a short introduction giving the circumstances out of which it arose, and followed by a postscript summarizing later legislation on the same subject. The book should be a useful reference work for students of French economic and social history.
- JENNINGS, I. *Problems of the new commonwealth*. Duke Univ. Commonwealth-Studies Center pub. no. 7. (Durham: Duke Univ. Press. London: Cambridge Univ. Press. 1958. Pp. xi, 114. \$2.50.)
- These three lectures were delivered in April 1957. The first is concerned with "Political Considerations," the second with "Economic Considerations," and the third with "Nationalism and Racism."
- KIRBY, E. S., ed. *Contemporary China—economic and social studies, documents, bibliography, chronology*. Vol. I, 1955. (Hong Kong: Hong Kong Univ. Press. New York and London: Oxford Univ. Press. 1956. Pp. xi, 264. HK\$25; \$5; 30s.)
- LITTMANN, K. *Zunehmende Staatstätigkeit und Wirtschaftliche Entwicklung*. (Cologne: Westdeutscher. 1957.)
- LOGANATHAN, C. *The private sector and economic development in the under-developed countries of Asia*. (Colombo: Ceylon Printers. 1957. Pp. 11.)
- MANKOV, A.-G. *Le mouvement des prix dans l'état russe au XVIème siècle*. (Paris: Sevpén. 1957. Pp. 302. 1.800 fr.)
- MANN, F. K. *Wirtschaftsgleichgewicht und Wirtschaftswachstum in den Vereinigten Staaten von Amerika*. Sonderschrift des IFO-Inst. f. Wirtschaftsforschung, no. 22. (Berlin: Duncker & Humblot. 1957. Pp. 27.)
- MILL, L. A. *Malaya: a political and economic appraisal*. (Minneapolis: Univ. of Minnesota Press. 1958. Pp. xi, 234. \$4.75.)
- MUDDATHIR, A. *Die Industrialisierung der wirtschaftlich unterentwickelten afrikanischen Länder u. ihre Auswirkungen auf die Weltwirtschaft*. Volkswirtschaft. Schriften, Vol. 31. (Berlin: Duncker & Humblot. 1957. Pp. 331. DM 26,—.)
- MYRDAL, G. *Rich lands and poor—the road to world prosperity*. World perspectives vol. 16. (New York: Harper. 1958. Pp. xx, 168. \$3.)
- This book is published in England under the title *Economic theory and under-developed regions*.
- NEF, J. U. *Cultural foundations of industrial civilization*. (New York: Cambridge Univ. Press. 1958. Pp. xiv, 163. \$4.)
- OHKAWA, K. in association with others. *The growth rate of the Japanese economy since 1878*. Inst. of Econ. Research, Hitotsubashi Univ., Econ. research ser. 1. (Tokyo: Kinokuniya Bookstore. 1957. Pp. xvii, 250.)
- OSTHUES, H. *Einkommensverhältnisse und private Kapitalbildung in Westdeutschland 1925-1953*. (Berlin: Duncker & Humblot. 1957.)
- PASSE, G. *Economies comparées de la France et de la Grande Bretagne*. (Paris: A. Fayard. 1957. 600 fr.)



- PENDLE, G. *Uruguay*. 2nd ed. (London: Oxford Univ. Press, for Royal Inst. of Internat. Affairs. 1957. Pp. vi, 107. 15s.)
- PHILIPPONNEAU. *Le problème breton et le programme d'action régionale*. (Paris: A. Colin. 1957. Pp. 180. 800 fr.)
- POTTINGER, D. T. *The French book trade in the Ancien Regime 1500-1791*. (Cambridge: Harvard Univ. Press. 1958. Pp. xiv, 363. \$7.50.)
- ROOS, C. F. *Dynamics of economic growth: the American economy, 1957-1975*. (New York: Econometric Inst. 1957. Pp. xxv, 374.)
- SMITH, A. G., JR. *Economic readjustment of an old cotton state—South Carolina 1820-1860*. (Columbia: Univ. of South Carolina Press. 1958. Pp. viii, 239.)
- STAPLETON, G. B. *The wealth of Nigeria*. (New York and London: Oxford Univ. Press. 1958. Pp. viii, 228. \$2.60; 16s.)
- TUCCI, U. *Lettres d'un marchand vénitien Andrea Berengo (1553-1556)*. École Pratique des Hautes Etudes, Centre de Recherches Hist., Affaires et gens d'affaires no. 10. (Paris: Sevpén. 1957. Pp. xii, 360. 2.200 fr.)
- WADIA, P. A. AND MERCHANT, K. T. *Our economic problem*. 5th ed. (Bombay: Vora & Co. 1957. Pp. xv, 825. \$4.50; 30s.)
- Automatisierung*. Hsg. vom Rationalisierungs-kuratorium der Deutschen Wirtschaft (RKW). (Munich: Carl Hanser. 1957.)
- Economic growth in the United States—its past and future*. Statement on nat. policy. (New York: Com. for Econ. Development. 1958. Pp. 63.)
- El liberalismo y la reforma en Mexico*. (Mexico, D. F.: Escuela Nacional de Econ., Univ. Nacional Autonoma de Mexico. 1957. Pp. viii, 789.)
- Contains chapters on: public finance, agriculture, industry, transportation and communication; and money and credit—in 19th century Mexico.
- L'économie du Mexique d'aujourd'hui*. (Paris: Inst. des Hautes Etudes de l'Amérique Latine. 1957. 800 fr.)
- The new India—progress through democracy*. (New York: Macmillan. 1958. Pp. x, 412.)
- By a study group at the request of the Planning Commission, Government of India. The group consisted of two representatives of the Ford Foundation and four members of the staff of the Planning Commission.

### Statistical Methods; Econometrics; Social Accounting

- BARKAY, R. M. *The public sector accounts of Israel 1948/49-1954/55*. Vol. 1, (Ch. 1-6; Appendix A); Vol. 2, (Appendixes B-G). (Jerusalem: Falk Project for Econ. Research in Israel and Central Bureau of Statistics. 1956. Pp. xiv, 155; ix, 207, mimeo. 1£1.)
- DUBOIS, P. H. *Multivariate correlational analysis*. (New York: Harper. 1957. Pp. xv, 202. \$4.50.)
- EGGLESTON, H. G. *Convexity*. Cambridge tracts in math. and math. physics, no. 47. (New York: Cambridge Univ. Press. 1958. Pp. viii, 136. \$4.)
- "My aim in writing this tract has been to provide a short introduction to this field of knowledge for the use of those starting research or for those working on other topics who feel the need to use and understand convexity." (From the preface.)
- MORSE, P. M. *Queues, inventories and maintenance—the analysis of operational systems with variable demand and supply*. Pub. in operations research no. 1. (New York: John Wiley. London: Chapman & Hall. 1958. Pp. ix, 202. \$6.50.)
- PLATT, H. *Input-Output Analyse*. (Meisenheim/Glan: Anton Hain. 1957. Pp. 121. Cloth, DM 16,—; paper, DM 14,—.)
- REYNAUD, P. L. *Du caractère relatif des statistiques de prix*. (Paris: Dalloz. 1957. Pp. 68. 300 fr.)
- XIMENES, V. T. *1956 income by counties in New Mexico*. New Mexico Stud. in bus. and econ. no. 5. (Albuquerque: Bur. of Bus. Research, Univ. of New Mexico. 1957. Pp. 48. \$2.)

*County and city data book 1956—a statistical abstract supplement.* Prepared under the direction of E. D. Goldfield, Statistical Reports Div., Bur. of the Census. (Washington: Supt. Docs. 1957. Pp. xxxi, 565. \$4.50.)

*The national economic accounts of the United States. Hearings before the Subcommittee on Economic Statistics of the Joint Economic Committee, 85th Cong., 1st sess., October 29 and 30, 1957.* (Washington: Supt. Docs. 1957. Pp. 302. 75¢.)

### Economic Systems; Planning and Reform; Cooperation

BOWEN, E. R. *The cooperative organization of consumers and its relation to producer and public organizations.* (Chicago: Cooperative League of the U.S.A. 1957. Pp. 87. \$1.)

BUCKINGHAM, W. S., JR. *Theoretical economic systems—a comparative analysis.* (New York: Ronald Press. 1958. Pp. viii, 518. \$7.)

CALCAGNO, A. E. *Nacionalización de servicios públicos y empresas.* (Buenos Aires: Ed. Raigal. 1957. Pp. 130.)

DUNAYEVSKAYA, R. *Marxism and freedom—from 1776 until today.* (New York: Bookman Assoc. 1958. Pp. 384. \$6.)

The book centers on the frequently neglected or misunderstood aspects of Marxian thought: its thorough-going commitment to the humanist tradition of all earlier revolutionary and socialist movements and of German classical philosophy. The crucial significance to Marx and Engels of this basic orientation is demonstrated by a close scrutiny of their works. The student of Marxism will appreciate the appendices presenting first English translations of important but little known philosophical statements by Marx and Lenin. The volume includes a preface by Herbert Marcuse.

FREI, R., ed. *Wirtschaftssysteme des Westens—Economic systems of the West—Systèmes économique de l'Occident.* Vol. I, Belgique, Denmark, Great Britain, Japan, Niederlande, Oesterreich, Spanien, Sweden. With summaries in English, French and German. (Basel: Kyklos. Tübingen: J. C. B. Mohr (Paul Siebeck). 1957. Pp. 247. DM 22.; paper DM 18.)

HARRIS, A. L. *Economics and social reform.* (New York: Harper. 1958. Pp. xvi, 357. \$5.)

HIRSCH, H. *Mengenplanung und Preisplanung in der Sowjetunion.* Veröffentlichungen d. List-Gesellschaft no. 5. (Tübingen: J. C. B. Mohr (Paul Siebeck). 1957. Pp. xii, 195. DM 24.50.)

OERTEL, R. R. *Das System der Sowjetwirtschaft.* (Berlin: Duncker & Humblot. 1957. Pp. 209. DM 18.60.)

PHILIP, A. *Le socialisme trahi.* (Paris: Plon. 1957. Pp. 256. 600 fr.)

### Business Fluctuations

ANTIER, D. *L'étude des flux et des stocks—méthode et applications de la comptabilité économique.* (Paris: Sté d'Edit. d'Enseignement Supérieur. 1957. Pp. 180. 750 fr.)

BACH, G. L. *Inflation—a study in economics, ethics, and politics.* Colver lectures at Brown University, Feb. 1957. (Providence: Brown Univ. Press. 1958. Pp. vii, 103. \$2.50.)

BURNS, A. F. *Prosperity without inflation.* Moorhouse I. X. Millar Lectures, delivered at Fordham University, October 1957. (New York: Fordham Univ. Press. 1957. Pp. ix, 88. \$2.)

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*January 1958 economic report of the President. Hearings before the Joint Economic Committee 85th Cong., 2nd sess., January 27-30, February 3-7, 10, 1958.* (Washington: Supt. Docs. 1958. Pp. viii, 498. \$1.50.)

*1958 Joint economic report.* Report of the Joint Economic Committee on the January 1958 Economic report of the President with supplemental and dissenting views and the Economic outlook for 1958, prepared by the committee staff, February 27, 1958, 85th Cong., 2nd sess. (Washington: Supt. Docs. 1958. Pp. iii, 52.)

## Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

FOUSEK, P. G. *Foreign central banking: the instruments of monetary policy.* (New York: Federal Reserve Bank of New York. 1957. Pp. 116.)

Most of the material in this booklet first appeared in the *Monthly Review of Credit and Business Conditions* of the Federal Reserve Bank of New York, but it has since been brought up to date and new sections have been added. The introduction is concerned with postwar trends in central banking techniques abroad, and the five succeeding chapters discuss in turn the various central banking instruments. The last chapter describes the money markets.

GOLDSMITH, R. W. *Financial intermediaries in the American economy since 1900.* Nat. Bur. Econ. Research stud. in capital formation and financing no. 3. (Princeton: Princeton Univ. Press. 1958. Pp. xxxv, 415. \$8.50.)

NUSSBAUM, A. *A history of the dollar.* (New York: Columbia Univ. Press. 1957. Pp. viii, 308. \$4.50.)

This volume presents the outstanding facts of our monetary history in relatively brief compass, but in interesting, authoritative, and effective fashion. The survey traverses the entire course of the development of the American monetary system, from colonial days to the present time. The study does not neglect the political and economic factors which underlie this monetary history, and it places due emphasis upon the contributions of such influential figures as Hamilton, Jefferson, Jackson, and Franklin D. Roosevelt. But it does not undertake to present a financial history of the United States, and hence it is not concerned with such problems as those involved in matters of taxation, tariff policy, budgetary adjustments, and the like. While it provides a simple and richly informative account of monetary history for the general reader, it is by no means irrelevant to the more intensive and specialized interests of the economist, the political scientist, or the historian. The text is fully documented, and there is appended an excellent bibliography and a list of official citations for all references to Congressional enactments.

SAYERS, R. S. *Lloyds Bank in the history of English banking.* (New York: Oxford Univ. Press. Oxford: Clarendon Press. 1957. Pp. xii, 381. \$5.60; 35s.)

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SACKS, S., EGAND, L. M. AND HELLMUTH, W. F., JR. *The Cleveland metropolitan area—a fiscal profile.* (Cleveland: Cleveland Metropolitan Services Comm. 1958. Pp. vii, 58. \$3.)

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- SAULNIER, R. J., HALCROW, H. G. AND JACOBY, N. H. *Federal lending and loan insurance*. (Princeton: Princeton Univ. Press, for Nat. Bur. Econ. Research. 1958. Pp. xxx, 566. \$12.)
- WISSLER, A. *Die Bedeutung der staatlichen Finanzpolitik für die Konjunkturstabilisierung*. (Berlin: Duncker & Humblot. 1957.)
- Federal expenditure policy for economic growth and stability. Hearings before the Subcommittee on Fiscal Policy of the Joint Economic Committee, 85th Cong. 1st sess., November 18-27, 1957.* (Washington: Supt. Docs. 1958. Pp. xi, 663. \$2.)
- Federal expenditure policy for economic growth and stability.* Papers submitted by panelists appearing before the Subcommittee on Fiscal Policy of the Joint Economic Committee, 85th Cong., 1st sess., November 5, 1957. (Washington: Supt. Docs. 1957. Pp. xix, 1203. \$3.25.)
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- The taxation of business income from foreign operations—studies in U. S., foreign, and international tax law.* Manag. report no. 2. (New York: American Management Assoc. 1958. Pp. 203.)
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- The authors have undertaken two tasks: "... the first to provide a simple account of British manufacturing industry in relation to European industries, the second, in itself the *raison d'être* of the first, to assess the effects of membership of European Free Trade Area on British trade and production. . . . We have endeavoured to marry the two approaches, to discuss the effects of freer trade industry by industry within the framework of an expanding European economy and, by adding together our detailed conclusions, to arrive at a general picture." (From the introduction.) The industry-by-industry part of the study includes chapters on metals and manufactures; general engineering; electrical engineering; motor vehicles; aircraft, shipbuilding and railway vehicles; chemicals and petroleum; textiles and clothing; and miscellaneous industries.
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WHITE, C. P. *Report on financing an expanded highway program in Tennessee.* (Knoxville: Bur. Bus. Econ. Research, Univ. of Tennessee. 1957. Pp. x, 75.)

WINSTON, A. P. *Judicial economics—(the doctrine of monopoly as set forth by judges of the U. S. Federal Courts in suits under the anti-trust laws).* (Austin: Ambrose Paré Winston. 1957. Pp. xiv, 175. \$1.)

As indicated in a parenthetical subtitle, this little book purports to deal with "The Doctrine of Monopoly as Set Forth by Judges of the U. S. Federal Courts in Suits under



the Anti-Trust Laws." But in point of fact it is in no sense a systematic or adequate treatment of either monopoly doctrine as developed by economists or of the course of judicial determinations related thereto. The author finds that "the two most striking characteristics of monopoly literature are fury and lack of knowledge"; and that "the great delusion," of which both economists and judges are victims, is that there can be such a thing as "price making by agreement." For support of this position, there is an analysis, in order, of the Trenton Potteries Case of 1927 (20 pages), the Addyston Pipe Case of 1899 (17 pages), and the Madison Oil Case of 1940 (80 pages), followed by repeated emphasis upon the controlling influences of demand—which, it is alleged, constitutes "a refutation of the whole doctrine that price may be fixed by agreement or combination of any sort." Whatever the shortcomings of antitrust policy as interpreted by the courts, the rejection by them of the sweeping contention that prices are controlled by market conditions under virtually any and all circumstances can scarcely be deemed to be one of these shortcomings.

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"The purpose of this study is to provide a comprehensive body of objective and reasonably up-to-date information on the degree of 'concentration' in the manufacturing segment of the industrial economy of America. All information is from the Census of Manufactures for 1954, 1947, and 1935 and from the 1950 and 1951 Annual Survey of Manufactures." The basic measures of concentration used are the proportions of total value of shipments and of total employment in a particular manufacturing category accounted for by the 4, 8, and 20 leading companies in that category. "Value added by manufacture is also used in measuring the share of total manufacturing activity accounted for by the 50 largest, 100 largest, 150 largest, and 200 largest manufacturing companies in the country." The various measures of concentration are applied in turn to the 20 major "2-digit" industry groupings, the 447 "4-digit" industry classes, and the more than 1000 "5-digit" product classes.

I. L. S.

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### Land Economics; Agricultural Economics; Economic Geography; Housing

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Each of the two authors contributed an essay; they jointly prepared an introductory and a concluding section. The two essays are: "Considerations in the Determination of an American Policy," by J. D. Brown; and "The Development of Human Resources in the Newly Industrializing Countries," by F. Harbison.

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## NOTES

A nominating committee consisting of O. H. Brownlee, Gerhard Colm, Frederick H. Harbison, J. Richard Huber, Margaret F. Myers and Simon Kuznets, chairman, has submitted the following slate of nominees for 1959 officers of the American Economic Association:

*President:* Arthur F. Burns

*Vice President:*

Kenneth E. Boulding  
Raymond T. Bowman  
Oskar Morgenstern  
Lloyd G. Reynolds

*Executive Committee:*

George L. Bach  
Howard R. Bowen  
Abram L. Harris  
Edward S. Shaw

*Representative, Social Science Research Council:*

Gardner Ackley

*Representative, American Council of Learned Societies:*

Frank H. Knight

The annual meeting of the Association will be held at the Palmer House, Chicago, Illinois, December 27-29, 1958.

### REPORT OF SURVEY OF MEMBERSHIP WITH REGARD TO AMERICAN ECONOMIC REVIEW

In September 1957 a questionnaire was sent by the managing editor to two samples of the membership of the Association. The purpose was to learn what use was being made of the various features of the *Review* and to elicit suggestions as to ways in which the journal might be of further service. The results of the survey are summarized as Exhibit I in connection with the report of the managing editor in the May 1958 (Proceedings) issue.

In general the responses indicate that the "service" sections of the *Review* (book reviews, titles of new books, titles of periodical articles, etc.) are widely used, that review and expository articles are regarded as useful features and that the leading articles and communications are ranked high, in general quality and usefulness, relative to those in other general economics journals.

In response to an invitation to offer suggestions, however, many replies stressed ways in which the articles in the *Review* might be improved. Apart from a few criticisms of the quality of the articles, the most frequently recurring suggestions were that the *Review* publish fewer specialized, technical and theoretical papers, and more papers of general interest, particularly on current problems of economic policy. Altogether, 65 respondents (out of 322) made suggestions of this sort. In addition, 36 respondents (including 11 of the 65 just mentioned) urged acceptance of fewer (or no) articles involving mathematical analysis.

The members of the board of editors believe that the point of view reflected in these particular replies is sufficiently widespread to call for some explanation of the board's present policy:

1. Since the primary purpose of the articles section of the journal is to encourage, and to



report the results of, economic research, most of the papers published will necessarily be technical, and often theoretical.

2. There is, however, no policy or prejudice on the part of the board against articles on current economic problems. In fact many of the articles now published are concerned, directly or indirectly, with such problems. (Many papers on current economic problems also appear regularly in the annual Proceedings volume of the *Review*.) The editors would warmly welcome the submission of a larger number of high-quality manuscripts in this category.

3. It should be remembered that, except for a few review, survey and expository articles, the editors do not "commission" papers on particular topics. We seek to select for publication the best papers of those voluntarily submitted. (By contrast, nearly all of the papers published in the Proceedings volume are commissioned in connection with the program of the annual meeting.) Hence the articles included in the four regular issues of the *Review* reflect the kinds of research on which members of the profession are engaged and the subjects in which they are interested.

4. The board would hesitate to change its policy of selecting leading articles from those submitted to one of commissioning articles in order to obtain more nontechnical papers or papers of broader interest. First, it believes that the primary emphasis in the four regular issues of the *Review* should continue to be on the stimulation of fundamental research and the reporting of its results. Second, competition for the opportunity of publication in the *Review* is more likely to result in high-quality papers than when articles are commissioned. Third, a large annual publication of the Association is already made up of papers that have, with a very few exceptions, been commissioned.

5. The objection to articles involving mathematical analysis is entirely understandable. Many, particularly the older, members of the profession cannot, or cannot easily, read them. On the other hand, an important segment of the profession does find mathematical techniques useful; and the editors do not believe that the official journal of the Association should be closed to them. However, the present policy is to: (1) limit somewhat the number of such articles accepted; (2) require that the mathematics should not be at an advanced level; (3) urge authors of such articles to make clear, for nonmathematical readers, the implications of the mathematical sections of the analysis. We generally do not accept mathematical articles if it is clear that the analysis could just as well be carried through without the use of mathematics.

#### REGIONAL FACULTY RESEARCH SEMINARS IN ECONOMICS

The Ford Foundation is initiating seven regional research seminars in economics in the summer of 1958. Participating in each seminar are ten selected faculty members teaching economics or business subjects at liberal arts colleges or other institutions of higher learning which do not offer a doctorate in economics. It is the purpose of this program to enhance the effectiveness of teaching and to encourage the preparation of research studies of significance.

The following seminars will be held: Government in the Economy, conducted by Charles E. Lindblom, at Yale University; Industrial Organization and Economic Theory, Fritz Machlup, Johns Hopkins University; International Economic Problems and Policies, Don D. Humphrey, Duke University; Recent Developments in the Theory of the Firm, Kenneth E. Boulding, University of Michigan; Public Finance and Fiscal Policy, Howard G. Schaller, Tulane University; Market Structure, Business Behavior, and Public Regulation, Francis M. Boddy, University of Minnesota; Money and Income, Lorie Tarshis and Edward S. Shaw, Stanford University.

#### FELLOWSHIP PROGRAM IN EAST ASIAN STUDIES FOR COLLEGE TEACHERS AT HARVARD UNIVERSITY

Six liberal arts college professors of social sciences and humanities have been awarded fellowships jointly by their institutions and by Harvard University's Center for East Asian Studies for the academic year 1958-59. They will undertake post-doctoral programs of studies which will enable them to offer new courses on Asia or to enrich existing ones with

more content about Eastern Asia. Persons interested in applying should write to Dr. Allan B. Cole, Curator, Fellowships in East Asian Studies, Center for East Asian Studies, Harvard University, 16 Dunster Street, Cambridge 38, Massachusetts.

### EXPERIMENT IN TEACHING

Columbia University's Graduate School of Business has undertaken a project to explore alternative methods of incorporating new valuable techniques into current business course offerings. It has been found that many useful new concepts and techniques are now untaught because faculty members and students lack the necessary technical training. A pilot project, under the direction of Alfred R. Oxenfeldt, professor of marketing, explores the use of a "team" to prepare separate materials for teachers and students in the field of pricing; these materials are intended to provide access to recent pricing developments such as the theory of games, mathematical probability models dealing with competitive bidding, marginal preference models which illuminate the nature of demand, and recent developments in psychology which help to explain businessmen's pricing objectives and consumers' responses to changes in prices. The team that will prepare these materials will include specialists in the fields of operations research and psychology and others who prove to be required.

### *Announcements*

Section K—Social and Economic Sciences—of the American Association for the Advancement of Science will hold sessions for contributed papers at the annual meeting of the AAAS in Washington, D.C., December 26-31, 1958. Association members interested in presenting a paper at these sessions should forward titles and abstracts not later than October 10 to the secretary of Section K, Donald P. Ray, National Academy of Economics and Political Science, George Washington University, Washington 6, D.C.

The Fund for Social Analysis, which has been recently established, is offering in 1958 a limited number of grants-in-aid for studies of problems posed by Marxist theory and its application. Grants will ordinarily range from \$500 to \$3,000 and may be requested for an entire project, or for any part, or for assistance in research, editing or publication. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

The next closing date for receipt of proposals in the Social Science Research Program of the National Science Foundation is October 1, 1958. Proposals received by that date will be evaluated in the fall. Approved grants will be activated in time for work to begin in the second semester or the summer of 1959. Proposals received after October 1, 1958 will be reviewed following the winter closing date of February 1, 1959, with activation of approved grants in the summer and fall of 1959. Inquiries should be addressed to National Science Foundation, Washington 25, D.C.

The Metropolitan Economic Association has elected the following officers for the year 1958-59: Robert D. Leiter, The City College of New York, president; Norris O. Johnson, The First National City Bank of New York, vice president; Harold B. Ehrlich, Ira Haupt & Co., secretary.

The Midwest Economics Association has elected the following officers for the year 1958-59: Harold M. Groves, University of Wisconsin, president; Ben W. Lewis, Oberlin College, president-elect; Victor E. Smith, Michigan State University, first vice president; Joseph T. McKenna, St. Louis University, second vice president; C. Woody Thompson, State University of Iowa, secretary-treasurer.

### *Visiting Foreign Scholars*

Lachlan McGregor of the University of Melbourne is visiting lecturer in economic

history at the University of Illinois. He will spend the coming academic year in Cambridge University, England.

A. W. Stonier of University College, London, will be visiting professor of economics at Duke University for the first semester 1958-59.

Brinley Thomas of Cardiff, Wales, will be visiting professor of economics at the University of Washington during the summer of 1958.

Herman Wold of the University of Uppsala will be visiting professor of economics at Columbia University in the academic year 1958-59.

### *Research Professorships*

The Brookings Institution has awarded National Research professorships for 1958-1959 to the following: Hugh G. J. Aitken, University of California, Riverside; William B. Gates, Jr., Williams College; Marion H. Gillim, Barnard College; James R. Nelson, Amherst College; Richard W. Perlman, Adelphi College; Jozo Tomasevich, San Francisco State College.

### *Deaths*

Norman S. Buchanan, director of the Social Sciences Division of the Rockefeller Foundation, died April 25, 1958.

Michael Dorizas, assistant professor in the department of Geography and Industry, Wharton School, University of Pennsylvania, died October 27, 1957.

Russell S. Grady, associate professor of commerce at the University of Kentucky, died January 6, 1958.

Harold W. Guest died July 8, 1957.

George L. Leffler, professor of finance at Pennsylvania State University, died February 14, 1958.

Sidney L. Miller died November 9, 1957.

Frank Parker, professor of finance in the Wharton School of Finance and Commerce, University of Pennsylvania, died February 14, 1958.

Charles F. Roos, president of the Econometric Institute, Inc., died January 7, 1958.

Walter W. Stewart, retired from the Institute for Advanced Study, and former member of the President's Council of Economic Advisers, died March 1958.

### *Appointments and Resignations*

Arthur J. Altmeyer, former U. S. Commissioner of Social Security, has been reappointed lecturer in economics and social security at the University of Wisconsin for the coming academic year.

Robert E. Asher has been appointed to the senior staff of the Brookings Institution.

Paul H. Banner has resigned from the Senate Subcommittee on Antitrust and Monopoly, and is now chairman, Research Committee, Southwestern and Western Trunk Line Railroads, St. Louis, Missouri.

Michael H. Belshaw has been appointed assistant professor of economics at Douglass College, Rutgers University.

William B. Bentsen, formerly at the University of Wisconsin and this past year at the London School of Economics, has been appointed assistant professor of economics at Beloit College.

Carl Biven has been appointed research associate in the School of Business Administration, Emory University.

Rudolph C. Blitz of Northwestern University has been appointed assistant professor of economics at Vanderbilt University.

Karl N. Bopp has resigned from the department of finance in the Wharton School, University of Pennsylvania.

William G. Bowen has been appointed lecturer in economics at Princeton University.

Ted R. Brannon, recently with Aramco in Saudi Arabia, has been appointed lecturer in management at the University of Florida.

Andrew F. Brimmer has been appointed assistant professor of economics at Michigan State University.

Robert O. Brown has been appointed assistant professor of commerce at the University of Kentucky.

Frederick M. Burgess has been lecturer in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term 1957-58.

Meyer L. Burstein has been appointed assistant professor of economics at Northwestern University.

Phillip D. Cagan, formerly of the University of Chicago, has been appointed associate professor of economics at Brown University.

John E. Candelet has been promoted to associate professor of economics at Trinity College.

Reynold E. Carlson of the International Bank for Reconstruction and Development has accepted an appointment as director, Graduate Training Program in Economic Development, and professor of economics at Vanderbilt University.

Pao L. Cheng of Michigan State University has been appointed associate professor of business finance in the School of Business Administration, University of Massachusetts.

Richard M. Cisek has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term 1957-58.

Willard W. Cochrane of the University of Minnesota will be visiting professor in the department of economics of the University of Chicago for the coming academic year.

Jerome B. Cohen has been appointed assistant dean of graduate studies in the Baruch School of Business, The City College.

Leslie Cookenboo, Jr. has resigned from the Rice Institute to accept a position with the Richfield Oil Corporation.

Frederick B. Cornish has been instructor in the department of geography and industry in the Wharton School, University of Pennsylvania in the spring term.

Clifton B. Cox has been appointed visiting research professor of business administration at Harvard University Graduate School of Business Administration.

John S. deBeers has resigned as director of the economic research department of the Government Development Bank for Puerto Rico and will engage in economic consulting in Washington, D.C.

Max W. Fletcher has been appointed assistant professor of economics in the College of Business Administration, University of Idaho.

Harold G. Fraine will return to the University of Wisconsin in the fall after a year's teaching in Indonesia. He will resume duties as director of a two-year research project sponsored by the Life Insurance Association of New York City.

M. Mason Gaffney has resigned from North Carolina State College to accept a position as associate professor of agricultural economics at the University of Missouri.

Hugh G. Hansen has resigned from the State University of Iowa to accept a position as chief of the Census of Irrigation, Bureau of the Census, Washington, D.C.

Joseph R. Hartley has been appointed assistant professor of transportation in the School of Business, Indiana University.

Millard Hastay, formerly of The City College and the National Bureau of Economic Research, has been appointed associate professor of economics at the State College of Washington.

O. E. Heskin has resigned from the University of Florida to accept a permanent position with the State Department in foreign service.

Bert G. Hickman has been appointed to the senior staff of the Brookings Institution. Donald F. Istvan has been appointed faculty lecturer in accounting in the School of Business, Indiana University.

Robert K. Jaedicke has been appointed assistant professor in business administration at the Harvard University Graduate School of Business Administration.

Ronald K. Jones has resigned as research associate in business administration at the Harvard University Graduate School of Business Administration.

A. D. H. Kaplan has recently been on a lecture tour in India. He is retiring from the senior staff of the Brookings Institution this month.

Richard D. Karfunkle, formerly of Pennsylvania State University, has been appointed staff economist in the operations research group of Chas. Pfizer and Co., Inc.

Robert M. Kaufman has resigned as senior economist, New York State Division of Housing, to accept an appointment as attorney, Department of Justice, Antitrust Division.

Richard deR. Kip has resigned as assistant professor in the department of insurance, Wharton School, University of Pennsylvania.

Arthur J. Kirsch has been appointed assistant professor of economics at Long Beach State College.

George Kleiner of the University of Illinois has spent the year on a Fulbright lectureship at the International Christian University, Tokyo.

Alex Kondonassis has been appointed economic historian at the University of Oklahoma.

Walter Krause, formerly with the International Cooperation Administration, has been appointed professor of economics at the State University of Iowa.

Harold Kuhn of Bryn Mawr College was visiting associate professor of economics at Princeton University during the second term of the current academic year.

Robert L. Lampman of the University of Washington has been appointed professor of economics at the University of Wisconsin.

Carl A. Lawrence has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term.

Louis Lefebvre has been appointed visiting assistant professor of economics at Stanford University for the summer quarter.

Robert Lekachman has been promoted to associate professor of economics and has been named acting executive officer of the department of economics, Barnard College. He is also serving as a consultant to the Fund for the Republic.

Abba P. Lerner has been appointed visiting distinguished professor of economics at Michigan State University, for the summer session.

Richard W. Lindholm has resigned from Michigan State University to become dean of the College of Business Administration at the University of Oregon.

Jan B. Luytjes has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, in the spring term.

Jesse W. Markham of Princeton University has a Ford Foundation research professorship for the year 1958-59.

Jacob Marschak, of Yale University, will be a Ford distinguished research professor in the Institute of Industrial Administration, Carnegie Institute of Technology, for the academic year 1958-59.

Bert C. McCammon, Jr. has been promoted to assistant professor of marketing in the School of Business, University of Indiana.

Robert E. McGarrah has been appointed associate professor of business administration at the Harvard University Graduate School of Business Administration.

Robert B. McKersie has resigned as research associate in business administration at the Harvard University Graduate School of Business Administration.

Nancy McKinney has been appointed instructor in economics at Carnegie Institute of Technology.

Walter J. Mead, after a year's leave with the Committee for Economic Development, has been appointed assistant professor of economics at the University of California at Santa Barbara.

Merton H. Miller has been promoted to associate professor of economics and industrial administration at Carnegie Institute of Technology.

Roger F. Miller of the University of California, Berkeley, has been appointed assistant professor of economics at the University of Wisconsin.

Basil Moore, formerly of The Johns Hopkins University, has been appointed assistant professor of economics at Wesleyan University.

Theodore Morgan of the University of Wisconsin will join the faculty in economics at Gadjah Mada University, Jogjakarta, Indonesia, this fall.

Richard F. Muth will be on leave from Resources for the Future to be visiting associate professor of economics at Vanderbilt University for the coming academic year.

Lawrence Nabers has been promoted to associate professor of economics at the University of Utah.

William H. Nicholls has been named chairman of the department of economics and business administration at Vanderbilt University.

Howard Nicholson of the University of Virginia has been appointed associate professor of economics at Clark University.

Miklos S. Nicolson is chairman of the business administration division of Phillips University, Enid, Oklahoma.

Ragnar Nurkse will hold the research professorship endowed by the Ford Foundation at Columbia University in the academic year 1958-59.

David W. Nylen has been appointed research associate in business administration at Harvard University Graduate School of Business Administration.

James R. O'Connor has been appointed instructor in economics at Barnard College. Ronald R. Olsen of the University of Ohio has been appointed assistant professor of labor economics in the department of economics at the University of Kansas.

Guy H. Orcutt of Harvard University has been appointed professor of economics at the University of Wisconsin.

Gardner Patterson has been visiting professor in the department of finance in the Wharton School, University of Pennsylvania, in the spring term. Effective July 1, he will succeed Dana G. Munro as director of the Woodrow Wilson School of Public and International Affairs, Princeton University.

Vernon W. Pherson has been appointed research associate in business administration at Harvard University Graduate School of Business Administration.

Joseph Pincus has resigned from the Division of Research and Analysis for American Republics, State Department, to take a position as tariff adviser, U.S. Operations Mission, Tegucigalpa, Honduras.

J. Stuart Prentice, professor emeritus of Middlebury College, has been visiting professor of economics at Dickinson College, Carlisle, Pa., in the past year.

Elton Rayack has been promoted to assistant professor of economics at the Pennsylvania State University.

Henry Rosovsky has been appointed acting assistant professor of economics at the University of California, Berkeley, for the academic year 1958-59.

Gerhard N. Rostvold has been promoted to associate professor of economics at Pomona College.

Julius Rubin has been appointed instructor in economics at Columbia University for the academic year 1958-59.

Ralph Russell has transferred his farm management consulting service to Connecticut and is working with dairy farmers in New Haven County.

Theodor Schuchat has been appointed executive director of Social Legislation Information Service, Washington, D.C.

Theodore W. Schultz of the University of Chicago is serving as chairman of the Research Advisory Board of the Committee for Economic Development.

Tibor Scitovsky has been appointed professor of economics at the University of California, Berkeley, for the academic year 1958-59.

Stanley J. Shapiro was instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term.

B. M. Stanfield, who is retired from Columbia University, is at Hobart and William Smith Colleges on a grant from the John Whitney Foundation. He is to be visiting professor at the University of Hawaii during the summer session.

Charles Stein, Jr. has been appointed lecturer in business administration at Harvard University Graduate School of Business Administration.

W. J. Stenason has been appointed director of economic research, Canadian Pacific Railway Company, Montreal.

Richard W. Stevens has been instructor in the department of insurance in the Wharton School, University of Pennsylvania, during the spring term.

George W. Stocking has resigned from the chairmanship of the department of economics and business administration at Vanderbilt University. He will continue as director of Vanderbilt's Institute of Research in the Social Sciences.

Robert H. Stroup has been appointed acting director of the Bureau of Business Research, University of Kentucky.

Joseph Taffet has been promoted to assistant professor of economics at The City College.

Frank A. Thornton has been promoted to assistant professor of economics at The City College.

William O. Thweatt has resigned from the Graduate Training Program in Economic Development, Vanderbilt University, to take a position as economic adviser to the International Cooperation Administration mission in Nepal.

Roger C. Van Tassel has been promoted to associate professor of economics at Clark University.

Jacob Viner of Princeton University has been selected by the American Council of Learned Societies to receive one of their Special Awards for distinguished scholarly accomplishments in the humanities and social sciences for the 1958-59 academic year.

Stanislaw S. Wasowski has been appointed assistant professor of economics at Duquesne University.

Charles C. Withers has been appointed lecturer on business administration at Harvard University Graduate School of Business Administration.

Harry D. Wolfe, who has been visiting professor of marketing at Columbia University and managing director of Behavioral Science Research Inc., will join the faculty of the School of Commerce, University of Wisconsin, in the fall.

James S. Worley of Princeton University has been appointed assistant professor of economics at Vanderbilt University.

John R. Yeager has been appointed lecturer on business administration at Harvard University Graduate School of Business Administration.

William P. Yohe of the University of Michigan has been appointed assistant professor of economics at Duke University.

Edward Zabel of the RAND Corporation has been appointed assistant professor at the University of Rochester.



## VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

### *Vacancies*

**Social studies:** Associate professor or professor of social studies (depending on experience and academic background), beginning September 1, 1958; salary \$5,976 or \$7,206. Master's degree with major training in economics required. Preparation should include several of the following areas: economic theory, consumer economics, labor problems, international trade, industrial organization, and international economic problems. At least thirty credit hours beyond the master's level are expected and promotion in rank would require considerable organized study at the doctoral level. Write: State University Teachers College, Department of Social Studies, Plattsburgh, N.Y.

**Accounting and statistics:** A small college in the Southwest will have a vacancy in September, 1958. Applicant must be qualified to teach advanced accounting, statistics, and related courses. Salary and rank depend upon training and experience. Write: Chairman, Department of Business Administration and Economics, Austin College, Sherman, Texas.

**Economics:** Ph.D. with a background in forecasting, general economic activity trends, gross national product. Especially desirable would be experience as economist for an airline.

**Cost analyst:** At least 4 years of experience with broad-brush cost analysis, such as preparation of cost inputs for weapons systems studies.

**Cost analyst:** Several years of experience with computer costs, with emphasis on nonmoney costs and frame time analysis. Experience with real time is desirable. Salaries commensurate with educational background and experience. These three positions are with a nonprofit research organization and carry outstanding fringe benefits. Please send detailed résumé to: Robert W. Frost, System Development Corporation, 2400 Colorado Avenue, Santa Monica, California.

**Business administration:** Openings in school of business administration in Washington, D.C., from assistant to full professorships. Primary interest in men of ability and initiative, willing to work hard to build rewarding career. Combination of teaching, administration, and work with business groups. Send detailed résumé of education and experience. P202

**Management:** Staff member wanted for fall, 1958, in private metropolitan university in Middle West. Must have Ph.D. Salary and rank depend on previous experience, publications, etc. Full-time salary can be supplemented by summer teaching. Opportunity exists for consulting work in the community. P206

